THURSDAY, JANUARY 31, 1878

TAIT'S "THERMODYNAMICS"

Sketch of Thermodynamics. By P. G. Tait, M.A., formerly Fellow of St. Peter's College, Cambridge, Professor of Natural Philosophy in the University of Edinburgh. Second Edition, revised and extended. (Edinburgh: David Douglas, 1877.)

THIS book, as we are told in the preface, has grown out of two articles contributed in 1864 by Prof. Tait to the *North British Review*. This journal, about that time, inserted a good many articles in which scientific subjects were discussed in scientific language, and in which, instead of the usual attempts to conciliate the unscientific reader by a series of relapses into irrelevant and incoherent writing, his attention was maintained by awakening a genuine interest in the subject.

The attempt was so far successful that the publishers of the *Review* were urged by men of science, especially engineers, to reprint these essays of Prof. Tait, but the *Review* itself soon afterwards became extinct.

Prof. Tait added to the two essays a mathematical sketch of the fundamental principles of thermodynamics, and in this form the book was published in 1868. In the present edition, though there are many additions and improvements, the form of the book is essentially the same.

Whether on account of these external circumstances, or from internal causes, it is impossible to compare this book either with so-called popular treatises or with those of a more technical kind.

In the popular treatise, whatever shreds of the science are allowed to appear, are exhibited in an exceedingly diffuse and attenuated form, apparently with the hope that the mental faculties of the reader, though they would reject any stronger food, may insensibly become saturated with scientific phraseology, provided it is diluted with a sufficient quantity of more familiar language. In this way, by simple reading, the student may become possessed of the phrases of the science without having been put to the trouble of thinking a single thought about it. The loss implied in such an acquisition can be estimated only by those who have been compelled to unlearn a science that they might at length begin to learn it.

The technical treatises do less harm, for no one ever reads them except under compulsion. From the establishment of the general equations to the end of the book, every page is full of symbols with indices and suffixes, so that there is not a paragraph of plain English on which the eye may rest.

Prof. Tait has not adopted either of these methods. He serves up his strong meat for grown men at the beginning of the book, without thinking it necessary to employ the language either of the nursery or of the school; while for younger students he has carefully boiled down the mathematical elements into the most concentrated form, and has placed the result at the end as a *bonne bouche*, so that the beginner may take it in all at once, and ruminate upon it at his leisure.

A considerable part of the book is devoted to the history of thermodynamics, and here it is evident that with Prof. Tait the names of the founders of his science call up the ideas, not so much of the scientific documents they have left behind them in our libraries, as of the men themselves, whether he recommends them to our reverence as masters in science, or bids us beware of them as tainted with error. There is no need of a garnish of anecdotes to enliven the dryness of science, for science has enough to do to restrain the strong human nature of the author, who is at no pains to conceal his own idiosyncrasies, or to smooth down the obtrusive antinomies of a vigorous mind into the featureless consistency of a conventional philosopher.

Thus, in the very first page of the book, he denounces all metaphysical methods of constructing physical science, and especially any *à priori* decisions as to what may have been or ought to have been. In the second page he does not indeed give us Aristotle's ten categories, but he lays down four of his own:—matter, force, position, and motion, to one of which he tells us, "it is evident that every distinct physical conception must be referred," and then before we have finished the page we are assured that heat does not belong to any of these four categories, but to a fifth, called energy.

This sort of writing, however unlike what we might expect from the conventional man of science, is the very thing to rouse the placid reader, and startle his thinking powers into action.

Prof. Tait next handles the caloric theory, but instead of merely showing up its weak points and then dismissing it with contempt, he puts fresh life into it by giving (in the new edition) a characteristic extract from Dr. Black's lectures, and proceeds to help the calorists out of some of their difficulties, by generously making over to them some excellent hints of his own.

The history of thermodynamics has an especial interest as the development of a science, within a short time and by a small number of men, from the condition of a vague anticipation of nature to that of a science with secure foundations, clear definitions, and distinct boundaries.

The earlier part of the history has already provoked a sufficient amount of discussion. We shall therefore confine our remarks to the methods employed for the advancement of the science by the three men who brought the theory to maturity.

Of the three founders of theoretical thermodynamics, Rankine availed himself to the greatest extent of the scientific use of the imagination. His imagination, however, though amply luxuriant, was strictly scientific. Whatever he imagined about molecular vortices, with their nuclei and atmospheres, was so clearly imaged in his mind's eye, that he, as a practical engineer, could see how it would work.

However intricate, therefore, the machinery might be which he imagined to exist in the minute parts of bodies, there was no danger of his going on to explain natural phenomena by any mode of action of this machinery which was not consistent with the general laws of mechanism. Hence, though the construction and distribution of his vortices may seem to us as complicated and arbitrary as the Cartesian system, his final deductions are simple, necessary, and consistent with facts.

Certain phenomena were to be explained. Rankine

set himself to imagine the mechanism by which they might be produced. Being an accomplished engineer, he succeeded in specifying a particular arrangement of mechanism competent to do the work, and also in predicting other properties of the mechanism which were afterwards found to be consistent with observed facts.

As long as the training of the naturalist enables him to trace the action only of particular material systems without giving him the power of dealing with the general properties of all such systems, he must proceed by the method so often described in histories of science—he must imagine model after model of hypothetical apparatus till he finds one which will do the required work. If this apparatus should afterwards be found capable of accounting for many of the known phenomena, and not demonstrably inconsistent with any of them, he is strongly tempted to conclude that his hypothesis is a fact, at least until an equally good rival hypothesis has been invented. Thus Rankine,[1] long after an explanation of the properties of gases had been founded on the theory of the collisions of molecules, published what he supposed to be a proof that the phenomena of heat were invariably due to steady closed streams of continuous fluid matter.

The scientific career of Rankine was marked by the gradual development of a singular power of bringing the most difficult investigations within the range of elementary methods. In his earlier papers, indeed, he appears as if battling with chaos, as he swims, or sinks, or wades, or creeps, or flies,

> "And through the palpable obscure finds out
> His uncouth way;"

but he soon begins to pave a broad and beaten way over the dark abyss, and his latest writings show such a power of bridging over the difficulties of science, that his premature death must have been almost as great a loss to the diffusion of science as it was to its advancement.

The chapter on thermodynamics in his book on the steam-engine was the first published treatise on the subject, and is the only expression of his views addressed directly to students.

In this book he has disencumbered himself to a great extent of the hypothesis of molecular vortices, and builds principally on observed facts, though he, in common with Clausius, makes several assumptions, some expressed as axioms, others implied in definitions, which seem to us anything but self-evident. As an example of Rankine's best style we may take the following definition :—

"A PERFECT GAS is a substance in such a condition that the total pressure exerted by any number of portions of it, at a given temperature, against the sides of a vessel in which they are inclosed, is the sum of the pressures which each portion would exert if inclosed in the vessel separately at the same temperature."

Here we can form a distinct conception of every clause of the definition, but when we come to Rankine's Second Law of Thermodynamics we find that though, as to literary form, it seems cast in the same mould, its actual meaning is inscrutable.

"*The Second Law of Thermodynamics.*—If the total actual heat of a homogeneous and uniformly hot substance be conceived to be divided into any number of equal parts, the effects of those parts in causing work to be performed are equal."

We find it difficult enough, even in 1878, to attach any distinct meaning to the total actual heat of a body, and still more to conceive this heat divided into equal parts, and to study the action of each of these parts, but as if our powers of deglutition were not yet sufficiently strained, Rankine follows this up with another statement of the same law, in which we have to assert our intuitive belief that—

"If the absolute temperature of any uniformly hot substance be divided into any number of equal parts, the effects of those parts in causing work to be performed are equal."

The student who thinks that he can form any idea of the meaning of this sentence is quite capable of explaining on thermodynamical principles what Mr. Tennyson says of the great Duke :—

> "Whose eighty winters freeze with one rebuke
> All great self-seekers trampling on the right."

Prof. Clausius does not ask us to believe quite so much about the heat in hot bodies. In his first memoir, indeed, he boldly dismisses one supposed variety of heat from the science. Latent heat, he tells us, "is not only, as its name imports, hidden from our perceptions, but has actually no existence;" "it has been converted into work."

But though Clausius thus gets rid of all the heat which, after entering a body, is expended in doing work, either exterior or interior, he allows a certain quantity to remain in the body as heat, and this remnant of what should have been utterly destroyed lives on in a sort of smouldering existence, breaking out now and then with just enough vigour to mar the scientific coherence of what might have been a well compacted system of thermodynamics.

Prof. Tait tells us :—

"The source of all this sort of speculation, which is as old as the time of Crawford and Irvine—and which was countenanced to a certain extent even by Rankine—is the assumption that bodies must contain a certain quantity of actual, or thermometric, heat. We are quite ignorant of the condition of energy in bodies generally. We know how much goes in, and how much comes out, and we know whether at entrance or exit it is in the form of heat or of work. But that is all."

If we define thermodynamics, as I think we may now do, as the investigation of the dynamical and thermal properties of bodies, deduced entirely from what are called the First and Second laws of Thermodynamics, without any hypotheses as to the molecular constitution of bodies, all speculations as to how much of the energy in a body is in the form of heat are quite out of place.

Prof. Tait, however, does not seem to have noticed that Prof. Clausius, in a footnote to his sixth memoir,[1] tells us what he means by the heat in a body. In the middle of a sentence we read :—

". . . . the heat actually present in a unit weight of the substance in question—in other words, the *vis viva* of its molecular motions" . . . .

Thus the doctrine that heat consists of the *vis viva* of

[1] On the Second Law of Thermodynamics. *Phil. Mag.* Oct. 1865, § 12, p. 244; but in his paper on the Thermal Energy of Molecular Vortices, *Trans. R. S. Edin.*, xxv. p. 557 [1869] he admits that the explanation of gaseous pressure by the impacts of molecules has been proved to be possible.

[1] Hirst's translation, p. 230, German edition, 1864, p. 258, "wirklich vorhandene Wärme, d.h. die lebendige Kraft seiner Molecularbewegungen."

molecular motions, and that it does not include the potential energy of molecular configuration—the most important doctrine, if true, in molecular science—is introduced in a footnote under cover of the unpretending German abbreviation "d.h."

J. CLERK MAXWELL

(*To be continued.*)

WOLF'S HISTORY OF ASTRONOMY

Geschichte der Astronomie. Von Rudolf Wolf. (München: R. Oldenbourg, 1877.)

THE "History of Astronomy," by Prof. Rudolf Wolf, of Zurich, a volume of 800 pages issued at a very moderate figure, is a contribution to the literature of the science of no ordinary value to the student. The production of such a work, involving an outline of the progress of astronomy from the earliest times to the present period, must have been a labour of great extent, requiring much research, notwithstanding the assistance that might be afforded by historical treatises previously in the hands of astronomers, and it is only due to Prof. Wolf to acknowledge the very able and complete manner in which he has accomplished the heavy task he had imposed upon himself some years since.

Those of our readers who may have been desirous of acquainting themselves with the general history of practical astronomy, and of familiarising themselves with the names and the nature of the services of the principal workers who have successively contributed to advance our knowledge of the science, more especially during the last three centuries, will, we think, have experienced difficulties which the volume before us is well calculated to obviate. The English reader has, it is true, Prof. Grant's classical work, the "History of Physical Astronomy," but there is much to be found in this volume, which it was hardly within the scope of Prof. Grant's work to incorporate. The writer of these lines very well remembers the fragmentary manner in which, some thirty-five years since, an English student of practical astronomy was under the necessity of obtaining information, more especially in private reading; and it is one of the most happy circumstances for the astronomical student of the present day that this want of suitable guides has been to a great extent removed, and his time therefore need not be wasted in a search for knowledge in second-rate or doubtful authorities, mistakes which he would be not infrequently led into thereby, being corrected only after vexatious delay and trouble.

Prof. Wolf divides his work into three books. The first deals with ancient astronomy and progress down to the fifteenth century, including theories, instruments, and writings. The second commences with "the reformation of astronomy" consequent on the publication of the great work of Copernicus, "De Revolutionibus Orbium Cœlestium," and treats of the advances made to the time of Newton; we find therefore in this division a summary of the labours of Galileo, Apian, Tycho Brahe, Kepler, Fabricius, Harriot, Hevelius, Huyghens, Gascoigne, and many others, including notices of the more important publications of the period, which are of interest and value. The third book treats of "the new astronomy," commencing with the discovery of universal gravitation and brings down the history of astronomical research and discovery to the present epoch. A very great amount of information is compressed into this last section of the work, and it is here that the care and research of the author are more particularly evidenced. There is much to be found in it, for which we should look in vain in a collective and compendious form elsewhere. It is well and accurately put together, the few errors we have remarked being of comparatively trifling nature; thus the Saturnian satellite *Tethys* appears as *Thetis*. The biographical notes, which are extended to contemporary astronomers, will be a welcome feature to many readers.

Students and others interested in the history of the most ancient of the sciences, who can command a sufficient knowledge of the German language, will find their advantage in the possession of Prof. Wolf's elaborate work, and we must not omit to say that that great desideratum in all works of the kind—a very sufficient index, at least as regards names mentioned in the history, will render it of easy reference.

J. R. HIND

(*To be continued.*)

OUR BOOK SHELF

Photographic Spectra. 136 *Photographs of Metallic, Gaseous, and other Spectra printed by the Permanent Autotype Process.* With Introduction, Description, &c., by J. R. Capron, F.R.A.S. (E. and F. N. Spon.)

WE gather from the author's introduction that he has chiefly aimed "to popularise a subject hitherto somewhat of a sealed book, confined to the laboratories of workers in special research." In this he should certainly succeed, though we think that his readers would not have been driven away if they had found a little more reference to the explanations of the various phenomena and the conclusions which have been drawn from them. As it is, the book is a good companion to Lecoq de Boisbaudran's "Spectres Lumineux." The spectra are sharp and clear, and the autotype process has lent itself well to this reproduction. The results are all the more commendable because Mr. Capron has not had advantages of considerable dispersion.

The account of the method employed is full and clear, and will make the book a very useful one to beginners in spectrography.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Sun-spots and Terrestrial Magnetism

PROF. PIAZZI SMYTH will no doubt welcome from any quarter a satisfactory answer to his question about the discrepancy between Dr. Wolf's sun-spot period, 11·1 years, and the supposed 10·5 years' period for the magnetic needle. If Mr. Smyth will refer to Prof. Loomis's chart of magnetic oscillations given in Prof. Balfour Stewart's paper on the subject in NATURE (vol. xvi. p. 10), he will see that there are exactly seven minimum-periods from 1787 to 1871, the mean of which is twelve years; the mean of the seven corresponding maximum-periods is 11·8 years. The true magnetic declination-period is then the mean of these, viz., 11·9 years. In exactly the same manner I have found that the mean period of sun-spots is 11·9 years. The auroral displays also have the same period.

But what is this period of 11·9 years? It is Jupiter's anomal-

istic year, or the time which elapses between two perihelion passages.

Prof. Wolf and Messrs. De la Rue, Stewart, and Loewy have all distinctly stated their belief that Jupiter is the chief cause in the production of sun-spots. This 11·9 years' period will then, I believe, remove what little doubt remains in some minds on the subject. Mr. John Allan Broun, F.R.S., has already shown in NATURE (vol. xvi. p. 62) that Dr. Wolf, to be consistent with his own relative numbers, ought to take a period of 11·94 years rather than one of 11·1, and while he himself favours a 10·5 years' period, he admits that there is no combination of planetary positions which would produce such.

I may perhaps be allowed to state here that in a paper I have just forwarded to the Royal Astronomical Society I have given what I believe are satisfactory reasons for the variations of these curves, and such as will enable us for the future to calculate with considerable accuracy the lengths of the periods, and guided by these reasons I have ventured to state my belief that we are now passing through a long minimum-period—one very similar to that which occurred at the close of the last century, and that the next *maximum* of sun-spots will fall in the year 1887.

I make this statement from an examination of the causes which produce the sun-spots; and it is so far remarkably confirmed by the behaviour of the magnetic needle. Mr. Broun, in NATURE, vol. xvii. p. 183, speaking of the very gradual manner in which the curve has been going to a minimum during the last three and a half years, remarks that "no such constant state of the sun's magnetic action will have been observed since the last years of the eighteenth century." To this I would add that immediately prior to the commencement of that long sun-spot minimum period, the mean of the magnetic interval, which occurred then (reckoning the interval from minimum to maximum), fell in the year 1785, and corresponded with the time of Jupiter's perihelion passage. Suppose now we represent this synchronism by 0, it will be found that the mean point in the next period lagged behind the perihelion 1·6 year; next, 5·3 years; next, 5·3 years. Having reached its maximum of lagging, in the next period it lagged 3·9 years; next, 1·2 year; next, 0·6 year; and in the last period the mean point fell in the year 1868, coinciding for the first time since 1785 with Jupiter's perihelion, and will be represented by 0. So that the magnetic oscillation in 1868 was just where it was in 1785. Is it not a natural inference, then, that we have commenced another cycle of magnetic declination?

What produces this lagging? This is a very important question, and one which I have reason to believe can be satisfactorily answered. B. G. JENKINS

January 19

On a Means for Converting the Heat Motion Possessed by Matter at Normal Temperature into Work

MY attention has just been directed to Mr. S. Tolver Preston's two papers in NATURE, vol. xvii. p. 31 and p. 202, in which he points out what appears to be an exception to the second law of thermodynamics. Some years ago I illustrated the same subject in a somewhat different manner by an experiment which is in some respects better suited for lecture purposes, and while the subject is beng considered may be useful to your readers.

Into the cork of a large bottle were fitted two glass tubes. One tube went to the bottom of the bottle, its upper end being terminated in a fine jet. The other tube only passed a short distance into the bottle, and its upper end terminated about an inch above the cork. To its lower end was fixed some pieces of blotting-paper, to its upper end was attached a small test-tube, the two being connected by means of a piece of india-rubber tube. Some water was put in the bottle and the cork fitted close in its place. The test-tube was then filled with ether or some volatile fluid, and fitted to the end of the india-rubber tube.

After the apparatus had attained a uniform temperature, the test-tube was inverted, so as to cause the ether to flow down the tube, and enter the bottle, where it spread itself over the blotting paper and, rapidly evaporating, produced a pressure inside the bottle. The addition of the ether vapour to the air already at atmospheric pressure, produced a pressure sufficient to force the water up the tube and out of the jet, causing it to rise to a considerable height into the air. At the beginning of the experiment all the apparatus was at a uniform temperature, and, according to the generally received opinion, ought to have been incapable of developing energy, yet on account of the ether vapour not being diffused through the system, it was able to do work at the expense of part of the heat in the system. JOHN AITKEN

Darroch, Falkirk, January 18

No Butterflies in Iceland

ALLOW me to point out that the lepidopterous insects said by Olafsen (not Olaffson) and N. (not R.) Mohr, to be found in Iceland, are not butterflies at all, but moths, as shown by the generic term *Phalæna* applied by each of those authors to every one of them—a term whose meaning your correspondent and his informant have failed to see. Those venerable authors, though dead and buried long before I ever heard of them, are old friends of mine, and I feel it incumbent on me to ask your readers not to impute to them this and other mistakes in Dr. Rae's letter. Whether there have been or still be butterflies in Iceland I am not competent to declare. I did not see any when I was there, but they may have got out of my way. I have, however, yet to learn that they exist in that country, and therefore I am inclined to believe Mr. McLachlan is right when he said that there are none. We have the testimony of the late Sir William Hooker ("Tour," &c., ed. 2, vol. i. p. 333) that no butterfly had ever been met with in Iceland up to 1809, the year in which he visited that island. Gliemann ("Geogr. Beschreib. Isl.," p. 165) in 1824 was unable to add to Mohr's list of twelve species of moths, and included no butterflies. If any of the latter have since been found it would be well for Dr. Rae to give his authority for the fact, otherwise his ingenious supposition that Icelandic butterflies and their larvæ have been destroyed since 1786, is unnecessary, and his "only possible way" of reconciling "perfectly opposite authorities" falls to the ground through the absence of any opposition on the part of the authorities he has cited. ALFRED NEWTON

Magdalene College, Cambridge, January 25

[Dr. Rae writes "to explain and correct a mistake which, by a little care and attention on my part could and should have been so easily avoided."]

On some Peculiar Points in the Insect-Fauna of Chili

MY friend Mr. Birchall misconstrued the meaning of my notes (NATURE, vol. xvii. p. 162) in a manner incomprehensible to me, when penning his own (p. 221). I, and many others, will share his "surprise" when he can produce any species of the genera *Carabus*, *Argynnis*, and *Colias*, or any of the *Limnophilidæ* from Australia or New Zealand. If he will do me the favour to again read my notes he will find that I refer solely to Palæarctic and Nearctic forms occurring in the Chilian sub-region and (unless by exception) nowhere else in the southern hemisphere.

Mr. Wallace's rebuke (p. 182) is to some extent merited. I *did not* give sufficient attention to the chapter in his work, to which he refers, in consequence of its *general* character. Mr. Wallace greatly extends the number of genera published by me as a sample. Some of these were perfectly familiar to me; others, I think, will fail to stand the test of minute application, partly because their distribution is more extended, partly because generic definitions are vague. I could add several interesting and marked genera. *Colias* may possibly be represented by more than one species on the Northern Andes; but it is the opinion of naturalists who, from practical acquaintance with the fauna of South America, and who, on a special point like this, are more competent than I to judge, that most of the very marked forms upon which I especially rely *do not* occur on the Northern Andes, which of late have been most assiduously worked by entomologists hunting insects for sale and perfectly alive to the value of such forms.

Mr. Darwin's theory alluded to by Mr. Birchall had not been overlooked. I was dealing with insects, and with a few marked genera, &c., of them, only. In plants there appears to be a tendency towards the appearance of analogous or identical forms all over the world when a sufficient altitude (varying according to the latitude) is reached. The laws that govern the distribution of the one ought equally to affect the other. Still the facts alluded to in my former letter remain unexplained. The southern portion of South America forms, as it were, an island, with a large admixture of Palæarctic and Nearctic faunistic elements existing in no other part of the southern hemisphere.

Lewisham, January 19 ROBERT MCLACHLAN

The Radiometer and its Lessons

PROF. OSBORNE REYNOLDS arranges his last letter (NATURE, vol. xvii. p. 220) under four numbered heads, and in the reply which I appear called on to make I will follow this division.

1. In the first section he says, "There is nothing in my earlier papers that is 'admittedly erroneous.' If there is error in these papers I am not aware of it." This is strange. In his first paper (*Proceedings*, Royal Society, vol. xxii. p. 40) Prof. Reynolds declares its object to be "to point out and to describe experiments to prove that these effects (the motions observed by Mr. Crookes) are the results of evaporation and condensation." Now they are not the results of evaporation and condensation: and it might have been seen, *ab initio*, that evaporation and condensation could have had nothing to do with them; for evaporation and condensation can only produce a temporary force, ceasing so soon as the distillation is complete, and cannot therefore be any part of the cause of a persistent force such as that detected by Mr. Crookes, which lasts for any length of time during which the heat is applied. In the same paper Prof. Reynolds further says, "The reason why Mr. Crookes did not obtain the same results within a less perfect vacuum [than that of the Sprengel pump], was because he had then too large a proportion of air, or non-condensing gas, mixed with the vapour, which was also not in a state of saturation." All this is manifest error. But this is not all, for *the whole of the theory* of those papers is erroneous; neither condensable vapour nor residual gas acts in the way described by Prof. Osborne Reynolds. In investigating the force arising from evaporation and condensation, he has overlooked the circumstance that the evaporation from the disc will keep back part of the vapour which would otherwise have reached it, and in investigating the effect of condensation he tacitly assumes that it does keep it back. Now in both cases the reverse of the assumption is what takes place, and he actually arrives at the absurd result that "if the opposite sides of a pith ball in vapour were in such different conditions [*i.e.*, one surface evaporating, the other condensing] the ball would be forced towards the colder side" (p. 404). His conclusion amounts to this: that the recoil of a cannon would be doubled if it were struck from behind by a missile at the same moment that it discharges an equal mass with equal velocity forwards! If he had not made these mistakes he would have got out only the forces which result from "the perceptible motion of the vapour," which he states "would be insensible" (p. 403), along with alterations in the general tension of the vapour *which would act equally on both sides of the disc.* Those errors vitiate the whole of his mathematical reasoning, so that the value for f which he gets, is not, as he supposes, "the force arising from evaporation," and his law connecting it with the heat falls to the ground. I have all along supposed, from Prof. Reynolds's having long ceased to mention his theory of evaporation and condensation, that he was aware of some of its errors.

The same error vitiates his reasoning in reference to the action of residual gas. If the error is corrected, and if, as he assumes, the gas coming up to the disc had been unpolarised (*i.e.* had brought to the disc equal numbers of molecules, and with equal velocities from all directions in front), his investigation *would only have given him an increase in the general pressure of the gas*, acting, as I pointed out in paragraph 5 of my first paper (*Phil. Mag.*, March, 1876, p. 179), equally on the front and back of the disc, except during the almost inappreciable instant of adjustment. *Prof. Osborne Reynolds therefore wholly missed the source of the persistent force with which Mr. Crookes's experiments deal.*

2. Prof. Osborne Reynolds next says that his second paper "does not conclude with his own expression of opinion that residual gas is not the cause of the force observed by Mr. Crookes." In reply I have only to quote the concluding words of that paper (*Phil. Mag.*, November, 1874, p. 391). After passing under review the two agencies (condensable vapour and residual gas), which he supposes are to be considered, he decides in favour of the former in the following words: "hence in such cases [*i.e.*, under the conditions which he supposed to prevail in Mr. Crookes's experiments] it seems to me that the effects must be due to the forces of condensation."

3. In the next section of his letter Prof. Reynolds states that Clausius and Maxwell "established the law that the only condition of thermal equilibrium in a gas is that of uniform temperature." I am not aware that they have ever established this law. The converse of it is obviously true, and has often been used, and the law itself has sometimes been assumed, but has never, so far as I know, been proved. I am, however, disposed to concur with those who think that it is probably true, and the conclusion in my paper on penetration (which is the reverse of that attributed to me by Prof. Reynolds) is in conformity with it. My conclusion is expressed in the following words (*Phil. Mag.*, December, 1877, § 4):—"Hence there must, in the cases that really arise, be some escape of heat which may be small but cannot vanish." And, I may remark, there will, according to my view, be two other sources of escape of heat, viz., conduction by diffusion, which was excluded from my investigation; and conduction by radiation, which was excluded both from Clausius's investigation (*Phil. Mag.*, June, 1862, p. 422, footnote) and from mine.

Prof. Osborne Reynolds a second time objects to my having excluded conduction when investigating the penetration of heat. As he attaches weight to authority he will perhaps be reconciled to my doing so, by the example of Clausius, as cited above, and by his justification of it in the following words (*loc. cit.*):—"In any case, however, it is allowable to consider separately each of these two ways in which heat moves."

Before passing from this subject I wish to take the opportunity of stating that Dr. Schuster's letter (NATURE, vol. xvii. p. 143) has satisfied me that I have hitherto erred in my estimate of the relative efficiency of penetration and conduction as agents for conveying heat. I am now convinced that penetration is usually feeble compared with conduction, and, in the figures representing De la Prevostaye and Desains' experiments, is to be sought in those portions of the curves which slope steeply downwards. The second part of my paper on penetration, that in which I apply the theory to experiment, will accordingly require considerable modification; and some of the statements which I made in my papers on Crookes's force will need amendment. The corrections that are required do not, however, affect any of the material parts of my theories of Crookes's force and of penetration, which depend essentially on the fact that there is a layer in the gas extending to a limited distance from a heater or cooler, throughout which the effects of the discontinuity in the gaseous motions at the surface will be felt, and that within that layer the stresses and the communication of heat follow special laws.

4. I have to express my great satisfaction at the explicit admission made by Prof. Osborne Reynolds in the fourth section of his letter, in the following sentences:—"There is one statement in Mr. Stoney's letter which is not erroneous. He says, 'I cannot find anywhere in Prof. Osborne Reynolds's writings an explanation of the thing to be explained, viz., that the stress in a Crookes's layer is different in one direction from what it is at right angles to that direction.' I [Prof. Osborne Reynolds] do not at all admit that this is 'the thing to be explained,' and I am quite sure that Mr. Stoney would find no explanation of it in my writings." This admission disposes finally of all controversy as to priority between us.

I need hardly, after this admission, follow Prof. Osborne Reynolds through the rest of his letter. His supposed invariable law "that the [Crookes's] force always tends to drive the vanes or bodies in the direction of their colder faces," does not seem to be true. A familar exception occurs when a spheroidal drop is supported over a platinum dish. The Crookes's force acting upon the platinum dish is equal to the weight of the drop, and acts downwards, *i.e.* in the direction of the *hottest* surface of the dish.

In applying his hypothetical case of a heater and cooler, A and B, within an envelope of intermediate temperature, to prove that "the force that causes the motion in the bodies cannot be due" to the stresses of my theory, he has overlooked the very obvious circumstance that the envelope, as well as B, is a cooler in reference to A, and the envelope, as well as A, is a heater in reference to B.

Prof. Reynolds observes that I have not defined polarisation. I described the kind of polarisation that exists in radiometers in my first two papers, and I will give a formal definition of the term as applied generally to gases in an article which I am preparing, and which I hope will be admitted into the pages of NATURE, giving as clear an account of my theory as I can, compatibly with brevity and the omission of mathematics.

The way in which Prof. Reynolds has excluded polarisation from his explanation is by assuming that the state of the gas close to the heated disc may be adequately represented by unpolarised gas of one temperature coming up to the disk, and unpolarised gas of another temperature leaving it, *i.e.*, by mole-

cules coming up to the heater in equal numbers and with equal velocities from *all* directions in front, and by molecules receding from the heater equally in all directions, although with augmented velocities. Under these circumstances there would be no difference in the pressure on the front and back of the disc, except during the very brief period of adjustment.

By making this assumption Prof. Reynolds leaves the part of Hamlet out of the play; for Crookes's force arises out of the very circumstance which has been omitted, viz., that the molecules that come up to the heater or cooler, arrive in the form of a rain which predominates in a definite direction, a direction which is normal to the heater and cooler in the simple case of their being parallel.

G. JOHNSTONE STONEY

A Double Rainbow

ON the 28th inst., at about 6.30 P.M. while myself and some ten or twelve other gentlemen were playing cricket, we were surprised to see what we all considered a most novel phenomenon—*a double rainbow.* The sky was cloudy and the weather was thundery. At the time referred to a shower of rain fell; the sun was about 10° above the horizon, shining out very brilliantly and reflecting upon the waters of St. Vincent's Gulf. Great wonder was expressed at the strange appearance, and much curiosity as to the cause.

The appearance was as follows:—There were two distinct and well-defined bows; the feet were united, but the apices were a considerable distance apart.

I am of opinion that the lower bow was caused by the direct light of the sun, while the light reflected from the sea produced the upper one.

THOMAS NOYÉ

Willunga, South Australia, November 30

SCIENCE IN TRAINING COLLEGES

THE Science and Art Department has just issued a circular having an important bearing on the teaching of science is to take in our training colleges, and therefore also in elementary schools.

The Lords of the Committee of Council on Education believe that the time has arrived when a special examination should be instituted at a period of the year better adapted to the training colleges than May, and that the nature of the examination and the payments made on the results should be modified to suit the circumstances of those colleges. They have therefore determined that in future a special examination in science shall be held in training colleges in December, immediately before the ordinary Christmas examination.

The examination will not be open to acting teachers. It will be held in those subjects only for which a special course of instruction is provided in the time-table of the College, and will be conducted by one of her Majesty's inspectors or by an officer of the Science and Art Department. Special committees will no longer be required for the training colleges; such returns as are necessary will be made by the principal. No student in a training college will be allowed to attend the May examinations of the Science and Art Department, except in physical geography in May, 1878.

The examination will be confined to the following nine subjects:—1. Mathematics. 2. Theoretical Mechanics. 3. Applied Mechanics. 4. Acoustics, Light, and Heat. 5. Magnetism and Electricity. 6. Inorganic Chemistry. including Practical Chemistry. 7. Animal Physiology. 8. Elementary Botany. 9. Physiography.

No student will be permitted to take up more than two subjects in any one year, and women will not be permitted to take more than one subject in a year.

The examination, except for mathematics, will be based on the syllabus of the several subjects given in the Science Directory; but the two stages, elementary and advanced, will be treated as a whole—one paper only being set. These examination papers will be framed much as the present May papers are framed, that is to say, with a certain number of compulsory questions and a certain number of optional questions, some of the latter being more difficult and more highly marked than the rest. Questions will also be set on the method of teaching various branches of the subject.

The successful students will be placed in the first or second class, the standard for a second class being as high as that of a *good* second class in the present advanced stage, and for the first class of a *good* first class in the advanced stage. All students who pass will be registered as qualified to earn payments on results and will receive certificates, but no prizes will be given. A payment of 3*l.* will be made on account of each first class, and 1*l.* 10*s.* on account of each second class obtained by a student in a training college.

In addition to the payments for theoretical chemistry, payments will be made for practical chemistry, of the same amounts and on the same conditions as those detailed in the Science Directory, § XLV. The circular contains an appendix with a syllabus of the subjects for mathematics in training colleges. We should advise all interested in this matter to obtain a copy of the circular.

SUN-SPOTS AND TERRESTRIAL MAGNETISM

I HAVE seen only to-day the number of NATURE (vol. xvii. p. 220) containing a letter from Prof. Piazzi Smyth on the above subject. I have also just now seen for the first time a communication from M. Faye to the French Academy of Sciences on July 30 last, in which there is a reference to the same subject; this I regret much, as M. Faye, through an incomplete acquaintance with my investigations, has drawn conclusions from one of them which are not exact. I shall at present refer only to the subject of Prof. Smyth's letter.

M. Faye considers the difference of the periods found by Dr. Lamont and myself for the diurnal oscillations of the magnetic needle (10·45 years) and by Dr. Wolf from the sun-spots (11·11 years), a sufficient proof that these cycles are not synchronous, and therefore that there is no causal connection between the two phenomena. Prof. Smyth asks an explanation relatively to this difference, upon the supposition that the two periods found are the true mean durations of the cycles for the respective phenomena. This supposition, however, is erroneous, and consequently M. Faye's deductions from it fail.

I have shown in a paper cited by M. Faye[1] that if we determine the epoch of the maximum diurnal oscillation of the needle from Cassini's observations made at Paris, and from Gilpin's observations made at London, we find it to have occurred in 1787·25. This epoch agrees very nearly with that deduced by Dr. Wolf for the maximum of sun-spots. If we compare this epoch with that of the last maximum which occurred for both phenomena near the end of 1870, we shall obtain a mean duration of 10·45 years, upon the assumption that eight cycles happened between these two epochs. There is no difference between Dr. Wolf and the magneticians excepting upon the question whether there were eight or only seven cycles. Dr. Lamont considers that the data existing between 1787 and 1818 are worthless for a decision upon this point, and by induction from the known cycles has concluded that three cycles must have occurred in the thirty-one years 1787 to 1818. Dr. Wolf believes there were only two. I have given the evidence which makes the existence of three extremely probable. This question has no relation whatever to the synchronism of the two phenomena.

If we could accept Dr. Wolf's view we should find, as I have shown, that the mean duration of a cycle for *both* phenomena since 1787 would be 11·94 years, while the sun-spot results for eight cycles determined by Dr.

[1] "On the Decennial Period," *Edinb. Trans.*, vol. xxvii.

Wolf during eighty years *before* 1787 give 10·23 years (or, if we take nine cycles, 10·43 years) for the mean duration. It is by mixing these two very different means that the Zurich astronomer finds 11·1 years, a mean that can evidently have no weight given to it. On the other hand, if Dr. Wolf is in error (as I believe he is) as to the existence of a maximum in 1797, the mean durations for the eighty years after, and for the eighty years before 1787 agree as nearly as the accuracy of the determinations for the beginning of the eighteenth century will admit.

I beg, then, to repeat that since the time when regular series of magnetic observations were commenced, till now, there is no difference whatever between Dr. Wolf and the magneticians as to the synchronism of the two phenomena.

Under these circumstances we come to the question—Are the sun-spot maxima and minima really synchronous with those of the magnetic diurnal oscillations? I have already said that this was so in 1787; and, considering only the cases for which we have complete materials for comparison, beginning with Schwabe's observations of sun-spots, it was so for the maxima of 1829, 1837, 1848, 1860, and 1870, and for the minima of 1824, 1833-4 (*q. p.*), 1844, 1856, 1866, and it is the case for the minimum at the present time. These coincidences are far more important, as showing a common cause, than may appear at first sight from this summary.

The successive oscillations of the sun-spot variations are not performed in equal times, neither are those of the magnetic variations. Was the duration of the oscillation for the sun-spots only eight years, as from the maximum in 1829 to that of 1837, so was that for the magnetic variations; did it amount to 12½ years nearly, for the sun-spots, as from the minimum of 1844 to that of 1856, this was also the case for the oscillations of the needle. Does the sun-spot variation proceed from a minimum to a maximum within about three and a half years as from 1833–4 to 1837, so does the magnetic oscillation. Does the sun-spot variation occupy nearly eight years between a maximum and the following minimum, as from 1848 to 1856, so does the diurnal oscillation of the needle.

It will be difficult to persuade physicists that, during nearly a century the sun-spot cycle has been shortened or lengthened, and the sun-spot variations have been accelerated or retarded, so nearly together with those of the diurnal oscillations of the magnet, by accidental coincidences. No doubt the admission of the existence of a causal connection between the two phenomena is opposed to the hypothesis, which many other facts render now wholly untenable, that the magnetic variations are due to the heating action of the sun.

I am obliged to Prof. Piazzi Smyth for giving me the occasion to explain a difficulty which has troubled others as well as himself. JOHN ALLAN BROUN

January 23

HENRI VICTOR REGNAULT

THE death of M. Becquerel, alluded to in our last issue, was followed on the 19th inst. by that of his friend and fellow-physicist, M. Regnault, whose name is associated so intimately with the elementary principles of our knowledge of heat. Henri Victor Regnault was born at Aix-la-Chapelle, July 21, 1810. His youth was spent in a hard battle against poverty in the effort to maintain not only himself, but his sister. While still a lad he wandered to Paris, and there obtained a position as assistant in the large drapery establishment known as Le Grand Coude, a name familiar at the present day to the lady visitors of Paris. Here ability and fidelity won for him friends, and at the age of twenty he was enabled to gratify his longings for a scientific education, and enter the École Polytechnique of Paris, the Alma Mater of so many famous French *savants*. After a course of two years here, in 1832 he entered upon active duties in the department of mines, and was absent from Paris for the next eight years. During the latter portion of this time he occupied a professor's chair at Lyons, and had a laboratory at his disposal. Here he embraced the opportunity to enter upon the field of research in organic chemistry, which had just sprung into existence as a branch of chemical science, under the hands of Liebig, Wöhler, Laurent, Dumas, and others. While many of the chemists of the day were engaged in theoretical disputes, and the battle between the electro-chemical theory and the newly-advocated type-theory was being hotly waged, Regnault devoted himself to the accumulation of the facts so sorely needed as foundation-stones by the disputants on both sides. Among his investigations at this time may be mentioned those on the composition of meconine, piperine, cantharidine, and other alkaloids, composition of pectic acid, identity of esquisetic acid with maleic acid, properties of naphthaline-sulpho-acid, &c. By the action of sulphuric anhydride on ethylene, he obtained the carbyl-sulphate, $C_2H_4S_2O_6$, which Magnus prepared later from alcohol. His most valuable researches, however, were on the halogen derivatives in the ethyl-group, especially interesting at the time of their appearance, when the theories of substitution were timidly being advocated. Among these compounds now familiar reagents to the organic chemists were mono-chloro-ethylene-chloride, $CH_2Cl.CHCl_2$, obtained by the action of chlorine on ethylene chloride, as well as the higher chlorinated derivatives, which offered one of the most striking instances of substitution. These were followed shortly after (1838) by the classical investigations on the actions of chlorine on ethyl-chloride C_2H_5Cl, in which one by one all of the hydrogen atoms were successively substituted by chlorine, until the limit, C_2Cl_6 was reached. Of importance also was the change of ether, $C_4H_{10}O$, into perchloroether, $C_4Cl_{10}O$. Another interesting series of preparations gave the substituted ethylenes by the action of alkalies on saturated halogen derivatives, ethylene-bromide for example, yielding vinyl-bromide, and hydrobromic acid:—

$$C_2H_4Br_2 + HKO = C_2H_3Br + KBr + H_2O.$$

By this method he discovered vinyl-bromide, vinyl-iodide, vinyl-chloride, dichlor-ethylene, $C_2H_2Cl_2$, and trichlor-ethylene, C_2HCl_3. Finally must be mentioned his discovery of carbon-tetrachloride, CCl_4, by leading chlorine into boiling chloroform. It is difficult for us at the present day to estimate the importance attached to these discoveries forty years ago, when every new fact was a glimmer of light to the organic chemist wandering in the dark, and few series of researches have stood the test of time so well as those carried out by Regnault in his Lyons laboratory. The faithful study of minute properties, and the careful attention to physical peculiarities, already gave evidence of the tendencies which were manifested more fully in another branch of science, and the appearance of his papers in the *Annales de Chimie et Physique* attracted the attention of the scientific world to the hitherto unknown provincial professor. In 1840 he was elected to replace Robiquet in the chemical section of the French Academy, and was appointed professor in the Ecole Polytechnique. In the following year he was elected to the chair of physics at the Collège de France. A few years later he became engineer-in-chief of mines, and in 1850 received the order of officer in the Legion of Honour.

With his removal to Paris the field of Regnault's investigations was changed. Like our own Faraday, after having obtained renown as a chemist, he suddenly turned physicist. He was scarcely established in Paris, when he began his famous series of experiments on specific heat. A few years previous, Dulong and Petit had determined the specific heat of a number of elements by

means of their calorimeter based on the method of cooling, and obtained data sufficiently accurate to warrant the establishment of their law that the product of the specific heat of an element and its atomic weight is a constant. Regnault, after having submitted their method to careful examination, found it useless for the exact determination of the specific heat of solids, and invented in its place the calorimeter bearing his name. It is based on the method of mixtures, viz., of heating a known weight of a substance to a known temperature, immersing it in a known weight of water at a known temperature, and determining the temperature of the mixture. With this apparatus, which is of a somewhat complicated character, in order to reduce to a minimum the possibilities of error, Regnault determined the specific heat of the liquid and solid elements, and of a great variety of compounds. From the comparison of these results he deduced the general law that for all compounds of the same formula and similar chemical constitution the product of the specific heat and the atomic weight is the same. He also confirmed, by his experiments, the hypothesis of Wöstyn, that the elements require the same amount of heat to be raised to a certain temperature, whether free or in combination, and showed, by his more exact results, the general truth of Dulong and Petit's law. In order to overcome the difficulties of determining the specific heat of gases, Regnault contrived an ingenious apparatus in which the gases passed through a spiral inclosed in a known weight of water. The volume of gas, its temperature on entering and leaving the apparatus, and the alteration in the temperature of the water supplied the necessary data. By this means he experimented with about thirty-five of the principal gases and vapours, and established the two important laws, 1, that the specific heat of any gas at constant pressure, whether simple or compound, is the same at all pressures and temperatures; and 2, that the specific heats of different simple gases are in the inverse ratio of their relative densities. Regnault prepared also an interesting table of the specific heats of various substances in the solid, liquid, and gaseous forms, from which it appears that the specific heat of the same body is commonly greater in the liquid than in the solid state, and always greater than in the gaseous state.

In his experiments upon heat Regnault was led to devise methods of measuring high temperatures accurately, and invented the well-known air thermometer, which can be used at all temperatures below that at which gas softens, and the mercury and hydrogen pyrometers, the latter of which permits the determination of the temperature in a furnace at any instant. In this connection he carried out also an elaborate series of experiments on the density and absolute expansion of mercury from 1° to 360°, the results of which, as tabulated, are of primary importance in the correction of thermometers and barometers, as well as in a multitude of physical experiments conducted with this liquid. Still more elaborate and exhaustive are the extensive series of determinations in connection with water, its specific heat at various temperatures, the tension of its vapour at various temperatures, and the latent heat of its vapour at various pressures, all of which were designed to serve as fundamental facts upon which to base the action of heat on water for industrial purposes. The specific heat of water was found to increase from 1 at 0° to 1·013 at 100° and 1·056 at 230°. For the determination of the tension of steam Regnault contrived a simple apparatus based on the fact that the maximum tension of steam at the boiling-point is equal to the external pressure, by the aid of which he was able to construct his table of tensions from 0·32 mill. at 32° to 20926 mill. at 230°.

The experiments with this apparatus were extended to a number of volatile liquids with the design of testing the truth of Dalton's supposition that the tension of the vapours of all liquids is the same at temperatures equally distant from their boiling points, and the results showed that although not a law, it was very nearly correct for small intervals of temperature in the neighbourhood of the boiling point. A variety of interesting results were also obtained from mixtures of gases and vapours, including the laws that a liquid does not give off a vapour of so high a tension in the presence of a permanent gas as in a vacuum, and that while the tension of the vapours of a mixture of liquids not dissolving each other is equal to the sum of the tensions of its liquids at the same temperature; on the contrary, the tension arising from a mixture of mutually solvent liquids is less than the sum of the individual tensions.

Perhaps the most important of Regnault's experimental investigations was that on the coefficient of expansion for air and other gases, as well as on the compressibility of gases. Dalton, Gay-Lussac, and Rudberg had obtained numbers for the coefficient of expansions differing widely from one another. It was reserved for Regnault to establish by the most delicate experiments the number ·03663 as the coefficient of expansion of air, and to show in addition that the law of Dalton and Gay-Lussac with regard to the regularity of expansion among gases was only approximately correct. A similar result was obtained in his investigations on the accuracy of Boyle and Mariotte's Law, on the compressibility of gases.

In addition to the chief lines of research alluded to, Regnault made a variety of interesting experiments on the phenomena produced by heat, and his hypsometer and hygrometer should be mentioned, on account of their simple and practical qualities. Some valuable investigations on the phenomena of respiration were made by him in connection with Reiset, and, together with Dumas, he carried out a lengthy research on illuminating gas.

His most valuable experimental results are collected together in vol. xxi. of the *Mémoires* of the French Academy, and a continuation is to be found in vol. xxvi. Regnault published, in 1847, a treatise on chemistry, which has survived numerous editions in France, and been translated into German, English, Dutch, and Italian.

In 1854 he was appointed director of the famous porcelain manufactory of Sèvres, and since that date much of his time has been devoted to improvements in ceramic processes. During the Franco-Prussian war he received a sad blow in the death, on the battle-field, of his second son, Henri Regnault, a promising artist, and universal favourite in Paris. He returned to his laboratory at Sèvres, after the declaration of peace, to find that the results of his last great research on the phenomena of heat accompanying the expansion of gases, derived from over 600 observations, had been destroyed. The announcement of this loss was his last communication to the scientific world. Since then, oppressed by grief and a victim to increasing infirmities, he has been forced to renounce his wonted pursuits. On the day when the gay artist world of Paris was celebrating the battle of Buzenval by laying wreaths on the grave of the young patriot-painter, the father was released from a long and painful illness by the hand of death.

As a scientific investigator, Regnault did not possess the brilliant originality of many of his fellow-physicists. It is as the patient, thorough, conscientious observer that he has won his way to the foremost rank. Possessing a wonderful ingenuity in the invention of mechanical appliances for the purposes of observation and a perfect familiarity with the mathematical department of physics, he has been enabled by means of his unflagging enthusiasm and unbending resolution to place the modern physicist and chemist in possession of an invaluable collection of constants, which at the present stage of science are in daily use not only in the laboratory of research, but for a large variety of industrial purposes.

T. H. N.

THE ORIGIN OF A LIMESTONE ROCK[1]

IN November, 1845, I laid before the Literary and Philosophical Society of Manchester my memoir "On some Microscopic Objects found in the Mud of the Levant and other Deposits; with Remarks on the Mode of Formation of Calcareous and Infusorial Siliceous Rocks," which memoir was published in vol. viii. of the second series of the Society's *Transactions*. In that memoir I sought to demonstrate two things—1st, that not only was Chalk made up of microscopic organisms, chiefly Foraminifera, as had recently been demonstrated by Ehrenberg, but that the fact was equally true and explanatory of the origin of all limestones except a few freshwater Travertins; 2nd, that some other extensive deposits, of submarine origin, in which no Foraminifera could now be detected, were not in the state in which they were originally accumulated. I concluded that Foraminifera had doubtless been present in them also, but that their calcareous shells had been dissolved out of them, and that this disappearance had been effected through the agency of water containing carbonic acid, at an early stage of the formation of these deposits. As is well known, this latter theory has been reproduced as a new one by some of the naturalists of the *Challenger* expedition, who have applied it to the explanation of phenomena of a substantially similar nature to those which I endeavoured to account for, in the same way, more than thirty years previously.

I am indebted for the slab of limestone forming the subject of this communication to my friends the Messrs. Patteson, the marble merchants of Oxford Street, Manchester. This slab appears to illustrate in an exquisite manner both the theories to which I have just referred. It is a specimen of the Bolland limestone, which, when sawn through, was found to contain a large concamerated Nautiloid shell more than twelve inches in diameter, which appears to me to have been a true Nautilus, though the section has not passed exactly through its centre so as to reveal any portion of its siphuncle. In the various parts of this slab we find the calcareous material exhibiting different conditions. Throughout the greater part of its substance we have evidence that it has originated in an accumulation of minute calcareous organisms—especially Foraminifera—but most of these are disintegrated and display vague outlines, a condition which I presume has resulted from the action of the carbonic acid already alluded to.

Scattered through the slab are numerous dark-coloured patches of a substance apparently identical with what the late Dr. Mantell designated Molluskite, and which he believed to be the remains of the soft animal substance of marine organisms. In many of these patches the Foraminiferous shells are better preserved than is the case with the rest of the matrix inclosing the large fossil shell. It appears as if this Molluskite had partially protected the calcareous Foraminifera from the solvent action which had disintegrated most of those forming the rest of the deposit.

But the most interesting features of the specimen are seen within the chambers of the Nautiloid shell. The Foraminiferous ooze has entered freely through the large, open mouth of the terminal chamber in which the animal resided and filled the entire cavity of that chamber. There is no doubt whatever as to the original identity in the character of the ooze thus inclosed within the shell and that which constitutes its investing matrix, though they now appear very different. The latter portion was freely permeated by water containing the solvent carbonic acid; hence the more or less complete disintegration of its Foraminiferous shells. But in the limestone inclosed within the large terminal chamber of the Nautiloid shell almost every Foraminifer is preserved in the most exquisite perfection. This is especially the case in the deeper part of the chamber, most remote from the mouth, as also in the instances of one or two of the more internal closed chambers, into which the mud has obtained entrance through small accidental fractures in the outer shellwall. It appears obvious to me that the thick calcareous shell of the Nautilus has protected the inclosed shells of the Foraminifera from the action of the solvent acid. I repeat that there is no room whatever for doubting that both portions of the Foraminiferous ooze, whether contained within or surrounding the Nautiloid shell, were originally in identical states. Microscopic observation makes this sufficiently plain. The differences now observable between them have arisen from changes which have taken place subsequent to their primary accumulation, and which changes have been due to differences of position; the one portion has been protected by the thick calcareous Nautiloid shell which would rob the water percolating through it of all its solvent carbonic acid, and thus preserve the contained Protozoa from destruction, and which protection would continue so long as any portion of the Nautiloid shellwall remained undissolved. The other, being unprotected, would be exposed to the full action of the solvent, which would percolate readily amongst the loosely aggregated microscopic organisms, and speedily act upon their fragile shells.

But there is a yet further feature in this interesting specimen requiring notice. The *closed* chambers of the Nautiloid shell are all filled with clear, crystalline, calcareous spar. The acidulated water, acting upon the calcareous Foraminifera of the ooze has become converted into a more or less saturated solution of carbonate of lime. This has passed, by percolation, through the shell of the Nautilus into its hollow chambers. Finding there suitable cavities it has gradually filled them up with a crystalline formation of calcareous spar, and which of course exhibits no traces of the minute organisms from which the calcareous matter was primarily derived. A similar crystallisation has filled up the smaller interspaces between the Foraminiferous atoms both inclosed within, and external to, the Nautilus, rendering the limestone capable of receiving a high polish.

If these explanations are as correct as I believe them to be, we have here the entire history of the origin of a limestone rock—from the first accumulation of the Foraminiferous ooze, as seen in the interior of the first large chamber of the Nautilus, to the deposition, in an inorganic mineral form, of the crystallised carbonate of lime within the closed chambers of the Nautilus, all being illustrated within the area of a slab of limestone little more than a foot in diameter.

[1] "On the Microscopic Conditions of a Slab from the Mountain Limestone of Bolland," by W. C. Williamson, F.R.S., Professor of Natural History in Owens College. Read before the Literary and Philosophical Society of Manchester, January 8.

THE LIQUEFACTION OF THE GASES

IN the recent article, in which the magnificent results recently obtained by MM. Cailletet and Pictet were detailed, we contented ourselves, in the account of the methods employed, by pointing out the extreme simplicity of that used by M. Cailletet. The simplicity, however, by no means takes away from the beauty of the method, and we now propose to return to it with a view of showing how closely it resembles in many of its details that employed by Dr. Andrews in his classical work on the continuity of the various states of matter.

Dr. Andrews, it will be remembered, in his experiments on the liquefaction of carbonic acid, used a glass tube capillary in the upper part, and in the remainder, of a bore just so wide that a column of mercury would remain in it when the tube was held in a vertical position. The gas to be operated on was confined to the narrow upper part of the tube by mercury, and the tube was tightly packed to an end piece of brass armed with a flange.

This permitted a water-tight junction with a corresponding end of a cold-drawn tube of copper of great strength. A similar end-piece was attached to the other extremity of this

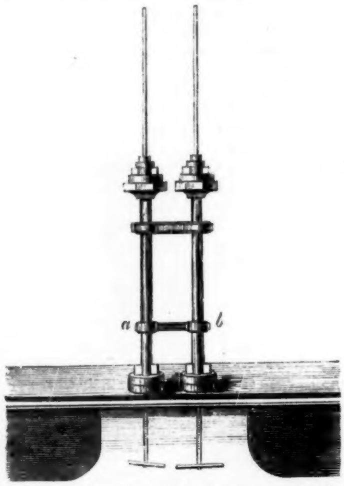

FIG. 1.—Two of Dr Andrews's tubes on a stand as in use

copper cylinder, and in the centre was a fine screw most carefully made and fitted, seven inches long, and packed so as to resist a pressure of 400 atmospheres or more.

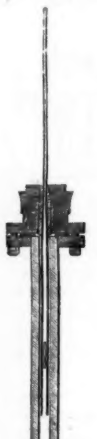

FIG. 2.—Section of Tube.

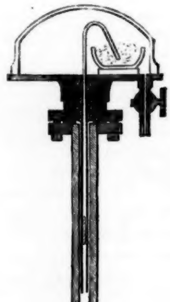

FIG. 3.—Arrangements for Utilising Low Temperatures.

When low temperatures as well as high pressures were required, the tube was bent, as shown in Fig. 3, and inserted in a freezing mixture.

In all these tubes the pressure is produced by screwing up the mercury into the capillary tube.

We have next to consider the phenomena which Dr. Andrews observed, taking carbonic acid as an example.

On partially liquefying the gas by pressure and changing the temperature, the surface of demarcation between the liquid and the gas became less and less distinguishable, the tube seemed to be filled with a homogeneous fluid which, when the pressure was suddenly diminished, or the temperature slightly lowered, broke up into striæ, Fig. 4.

A cloud was also formed if the temperature were allowed to fall a little below the "critical point" 30°·92 C., showing the formation of liquid particles, Fig. 5.

We may now pass to M. Cailletet's method and the phenomena he observes, Fig. 6, for which we are indebted to the courtesy of the editor of *La Nature*, represents the great apparatus which M. Cailletet has constructed at his works of Châtillon-sur-Seine.

The apparatus is composed of a hollow steel cylinder A solidly fixed to a cast-iron frame by means of the hoops B B. A cylindrical shaft of soft steel acting the part of a plunger enters this cylinder, which is filled with water. The opposite extremity of the shaft is

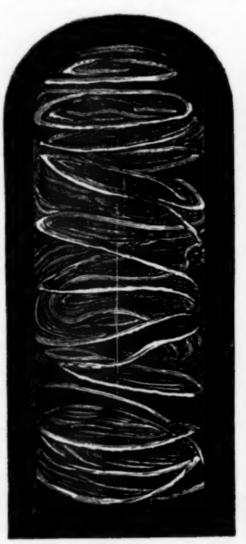

FIG. 4.—Striæ.

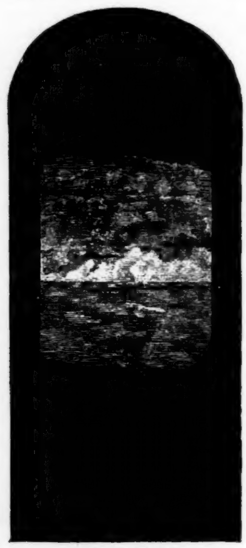

FIG. 5.—Cloud.

terminated by a square-threaded screw, which traverses the bronze nut F, fixed in the centre of the fly-wheel M. According to the direction given to the fly-wheel by means of the handles with which it is provided, the plunger may be advanced into or withdrawn from the axis of the body of the pump. A leather packing prevents the compressed liquid from escaping from the cylinder.

In order to introduce the water or the liquid to be compressed, it is poured into the glass vessel G, which is in communication with the interior of the apparatus; a steel screw with conical point closes the narrow pipe through which the liquid passes. This screw is terminated by a small fly-wheel O, with handles. This arrangement permits of suddenly expanding the compressed gases, and seeing the cloud produced in the capillary tube where the gas under experiment is contained. (This tube is represented in the centre of the glass envelope, *m*.) The cloud is formed under the influence of the external cold produced by the sudden expansion, a certain sign of the liquefaction or even of the solidification of the gases regarded hitherto as permanent. *a* is a hollow steel reservoir

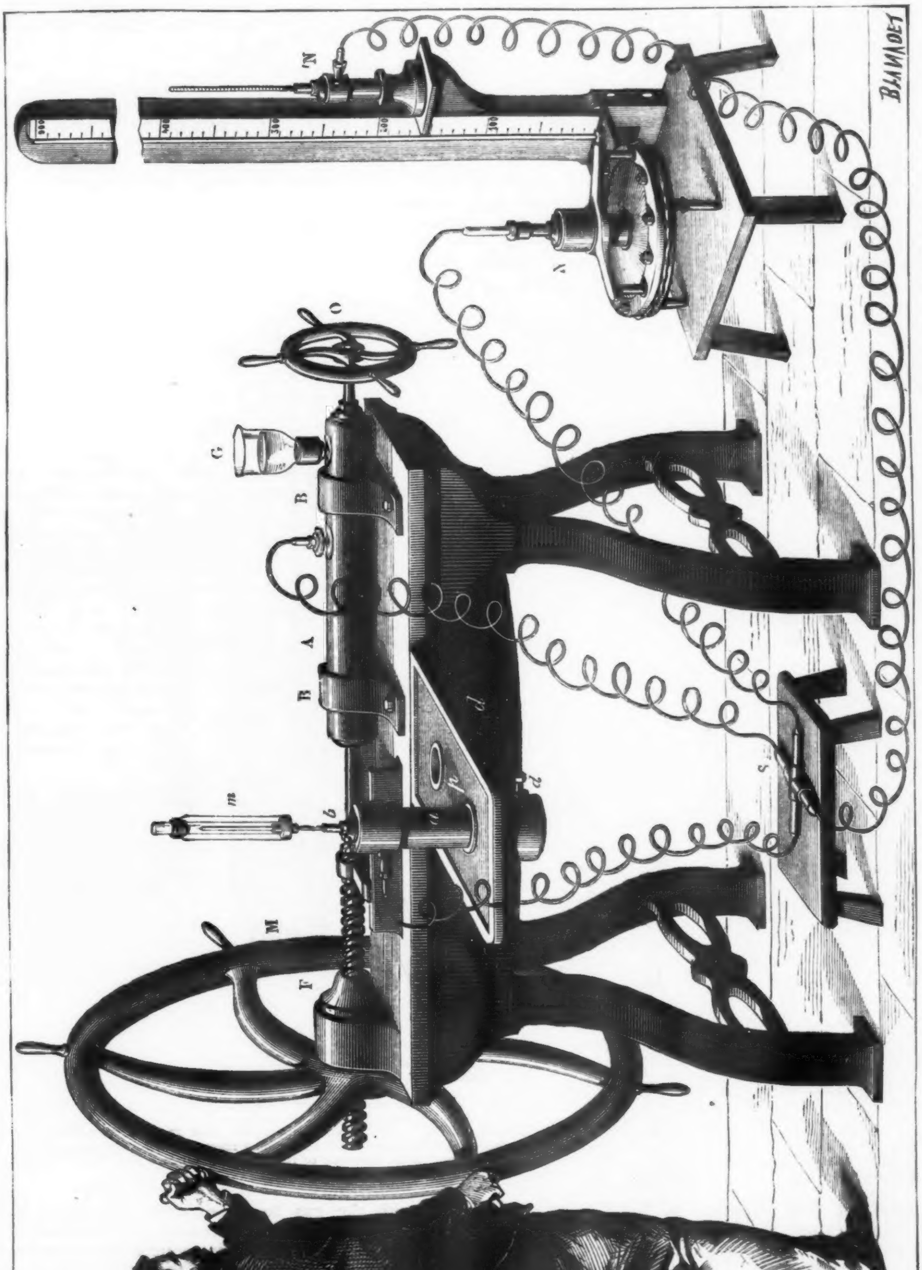

FIG 6.—Great apparatus of M. Cailletet for the liquefaction of the gases A, screw-press for compression; *m*, glass cylinder, containing the glass tube in which the gas is liquefied.

capable of supporting a pressure of from 900 to 1,000 atmospheres; it is connected with the compression apparatus by a capillary metallic tube. The water, under the action of the piston, arrives in this reservoir, *a*, and acts upon the mercury which compresses the gas. *b* represents the tube which connects this with the glass intended to contain the gas under experiment. A nut serves to fix this piece to the upper part of the reservoir. Fig. 7 shows this arrangement in half-size.

m is a glass cover containing a cylinder of the same material, in the middle of which is a small tube in which the liquefaction of the gas takes place. This capillary tube may be surrounded with refrigerating mixtures or with liquid protoxide of nitrogen. The exterior cover, *m*, concentric with the first, and containing substances strongly absorbent of moisture, prevents the deposit of ice or vapour on the cooled tube in which the experiments are made. *p* is a cast-iron tablet intended to support the reservoir, *a*; the screws, *d d*, enable the reservoir to be raised or lowered for the spectroscopic examination or the projection of the experiments. An arrangement, S, unites the capillary metallic tubes which transmit the pressure to the various parts of the apparatus. N is a modified Thomasset manometer verified by means of an air manometer established on the side of a hill near the laboratory of Châtillon-sur-Seine. N′ represents a glass manometer which serves to control the indications of the mercury apparatus.

It is a fortunate thing that the students of science in France have not been forgotten by M. Cailletet. He has not only devised the instrument above described for his own work, but he has occupied himself with a small lecture or laboratory apparatus which M. Ducretet has constructed according to his directions. It is an exact copy of the part, *a*, *b*, *m* of the apparatus of Châtillon-sur-Seine. The bell-glass alone is modified. The screw-press is, moreover, replaced by an easily-worked pump. In Fig. 8 T′ T is a glass tube filled with the gas to be compressed; the tube has been traversed by the gas until air has been entirely excluded; for this purpose it is placed in a horizontal position. When it is filled with the gas to be experimented on it is hermetically sealed at its extremity, *p*, closed with the finger at the other end, and introduced vertically into the iron apparatus as represented in the figure. It is inserted into a cylindrical cistern containing mercury. The upper part of the tube is enveloped in a glass envelope, M, filled with a refrigerating mixture. The whole is enveloped in a glass jar, G. The tube, T U is connected with a compressing pump, which is worked with the hand. The water compressed by the pump acts on the upper part of the mercury indicated in the figure by horizontal lines. This mercury is driven back into the

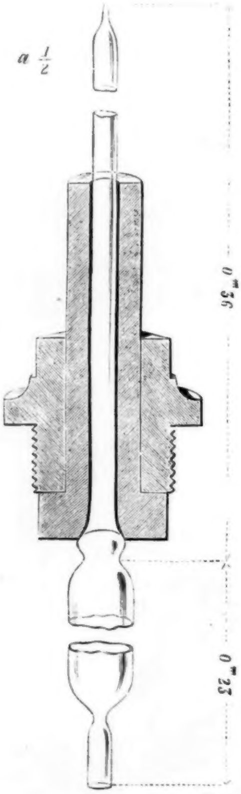

FIG. 7.

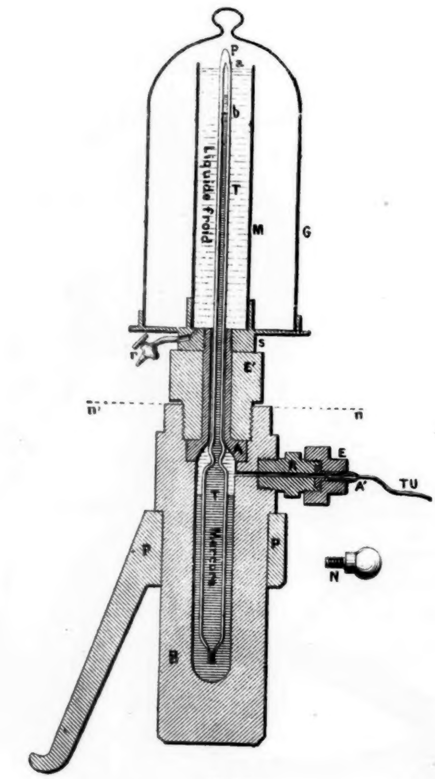

FIG. 8

FIG. 7.—Glass tube with thick sides in which the liquefaction of the gases is effected in M. Cailletet's apparatus. The gas is compressed in the upper part of the tube by the ascent of a column of mercury placed in connection with a screw-press acting on a mass of water. It condenses in a liquid drop or into mist under the action of expansion. This glass tube is enveloped in an envelope of the same substance containing the refrigerating mixture. See the centre of the tube *m* in Fig. 2. FIG. 8.—Small apparatus for the liquefaction of gases.

tube T T; it reduces the space *a b* occupied by the gas, and is soon surmounted by droplets of the compressed gas, which unite into a little mass of liquid, *b*.

The following are the parts of the apparatus:—B, a block of malleable iron with strongly-resisting walls; E′, E, screw nuts which may be unscrewed to arrange the apparatus before using it; P P, very solid tripod which receives the apparatus; S, support of the bell G and the envelope M; N supplementary screw intended to close the hole in the joint R when the mercury is poured into the apparatus.

OUR ASTRONOMICAL COLUMN

THE ROYAL OBSERVATORY, CAPE OF GOOD HOPE.—Since the appointment of Mr. Stone to the directorship of this establishment, in 1870, not only have all arrears of observations with the transit-circle, first brought into use in 1855, been reduced and published, but Mr. Stone has lately issued the results of observations taken in 1875, and has thereby overtaken the position of publications of the Royal Observatory, Greenwich, and the Radcliffe Observatory, Oxford, which have been conspicuous amongst astronomical establishments for the expedition with which the great mass of work involved in the reduction of the observations has been performed, and the results given to the scientific public.

The chief work of the year was the continuation of the general re-observation of the stars in the *Cœlum Australe Stelliferum* of Lacaille, attention in 1875 having been directed to those stars lying between 145° and 155° of north polar distance at the present epoch, all of which appear to have been observed, usually three times in both elements, together with a number of other stars in the same zone, which, though not generally much below the seventh magnitude, were not observed by Lacaille. Mr. Stone mentions that stars within limits of N.P.D 135°–145° were observed in 1876, and stars between 125°–135° in 1877.

Should it be deemed advisable shortly to form another general catalogue of stars, similar to the British Association Catalogue, say to stars of the seventh magnitude inclusive, Mr. Stone's recent volumes will be of the utmost value in extending the precision now attainable for such stars in the northern hemisphere to the southern heavens, not only as regards positions for the present epoch, but in the determination of proper motions of a considerable number of stars by comparison with Taylor's catalogues, which have not yet been systematically examined for that purpose. And we will take this opportunity of expressing the hope that if another catalogue like the B.A.C. should be undertaken, the time, labour, and expense involved in the preparation of so-called star-constants may be avoided, and attention paid instead to a more general and systematic investigation of proper motions, which, it can hardly be doubted, must lead to results of great interest and importance.

THE TOTAL SOLAR ECLIPSE OF JULY 29.—It was mentioned in NATURE last week that facilities would be afforded to intending observers of this phenomenon near Denver, Colorado, one of the chief places included in the belt of totality in the United States, and situated on the Pacific line of railway. By the elements of the *Nautical Almanac* the track of central eclipse appears to pass about twenty-five miles south of Denver, assuming its longitude from Greenwich to be 7h. 0m. 20s. W., and latitude 39° 48′, and at Denver the total phase commences at 3h. 28m. 14s. local mean time, and continues 2m. 45s., with the sun at an altitude of 42°; the circumstances by the elements of the American ephemeris are almost identical, as indeed was to be expected seeing that the moon's place in the latter work differs from her place in the *Nautical Almanac* by only + 3″·4 in R.A. and + 1″·0 in decl. and the sun's place by − 1″·1 in R.A. and + 0″·3 in decl., while the semi-diameters employed are each less by about 2″. In the American ephemeris the lunar tables of Peirce and the solar tables of Hansen are employed.

The northern and southern limits of totality in the eclipse of July 29, with the duration of total phase upon the central line, for nearly the whole track across the North American continent will be found at p. 400 of the *Nautical Almanac* for 1878.

CHEMICAL NOTES

TEMPERATURE OF FLAMES.—In the *Gazetta chimica Italiana* an account is given by F. Rosetti of some experiments on the above subject. To examine the temperatures he employs a thermo-electric element consisting of an iron and a platinum wire wound closely together and connected with a galvanometer. This latter was graduated to various temperatures by observing the deviation consequent on bringing the element in contact with a copper cylinder heated to known temperatures; these being determined by introducing the cylinder into a calorimeter. With such an arrangement he has investigated the flame of a Bunsen's burner, finding that in the same horizontal strata there were but slight alterations in the temperature, with the exception of the dark interior portion. Thus, where the external envelope showed 1,350°, the violet portion of the flame was 1,250°, the blue 1,200°, but the internal portion much lower, its temperature gradually decreasing from the base of the flame upwards. A flame produced by the combustion of a mixture of two volumes of illuminating gas and three volumes of carbonic oxide, showed a temperature of 1,000°.

STARCH IN PLANTS.—Botanists have hitherto held that all the starch in the chlorophyll cells of the leaves of plants is a product of the direct assimilation of carbon dioxide and water, basing this belief on the fact that the starch in these cells disappears when the plants are deprived of the power of assimilating carbon dioxide, but reappears on their exposure to light in an atmosphere containing that substance. Prof. Bohn, of Vienna, in a recent number of the *Deut. chem. Ber.*; throws some doubt on this conclusion by experiments he has made on the leaves of the scarlet runner. His results show that if the primordial leaves of this plant are shaded from light, the starch at first entirely disappears; after a few weeks, however, the chlorophyll cells of these shaded leaves show almost as high a percentage of starch as the parts of the plant which have been exposed to light. These observations demonstrate, therefore, that starch can be formed in the leaves from matter which has already been assimilated, and has entered into the leaf after its removal from the sunlight.

SIPYLITE, A NEW MINERAL CONTAINING NIOBIUM.—Mr. Mallett has found this mineral among some quantities of allanite from Amhurst county, Virginia. A few crystals have been obtained, but as they are of rather imperfect nature the measurement of the angles has only been attempted in a rough manner. The mineral in the mass was of a brownish black nature, but in thin plates it exhibited a reddish-brown colour, and possesses a pseudo-metallic lustre. The hardness is estimated at about 6, and the specific gravity as equal to 4·89. From the results of analyses Mr. Mallet considers that placing together the acid oxides of niobium, tantalum, tungsten, tin, and zirconium, reducing the basic oxides to equivalent amounts of dyad oxides, and eliminating the water, the following ratio may be obtained:—$R''O : M^v_2O_5 = 221 : 100$, leading to the formula $R''_3M^v_2O_8 \cdot 4R''_2M^v_2O_7$, that is a single group of orthoniobate associated with four of pyroniobate. If the water be taken into account in the calculation and considered basic, then placing it on the same footing as the dyad oxides, we should have the

relation $R''O : M^v{}_2O_5 = 311 : 100$, or nearly $3 : 1$, thus giving the simple formula $R''_3M^v{}_2O_8$; this latter the author considers the more probable. Whatever formula, however, may be taken for the mineral it differs from niobates hitherto described, the one view making it an approach to a simple pyroniobate, the other making it an orthosalt like Fergusonite, but partially acid in character, or containing basic hydrogen.

MOLYBDENUM.—The atomic weight of this metal has hitherto been quite uncertain, some chemists regarding it as 96, others as 92. Fresenius, the leading authority in analytical chemistry, has always adopted the latter number. Prof. Rammelsberg, of Berlin, has lately settled the question by careful experiments on the reduction of molybdic acid in an atmosphere of hydrogen, and has found 96 to be the correct atomic weight, – 96·18 being the exact number obtained. Taking this number as a basis, he has sought to solve the problem of the composition of the yellow phospho-molybdate of ammonium, which is used generally for the determination of phosphoric acid, and the exact formula of which has never been satisfactorily determined. A large number of analyses of the ammonium salt and the corresponding potassium salt show that the composition is undoubtedly

$$3(NH_4)_2O + P_2O_5 + 22MoO_3 + 12H_2O.$$

RELATIONS BETWEEN THE VOLUMES OF SILVER SALTS.—H. Schröder communicates an interesting series of observations on this subject in the *Berichte der deutschen chemischen Gesellschaft*, for November, from which it appears that the atomic volumes (*i.e.*, the quotient resulting from the division of the molecular weight by the specific gravity) of these salts are all simple multiples of the atomic volume of silver, or rather of its half atomic volume, 5·14. In the fatty series an accession of CH_2 to a compound increases the atomic volume by $3 \times 5{\cdot}14$. For example:—

$$C_2H_3AgO_2 = 10 \times 5{\cdot}14 = 51{\cdot}4$$
$$C_3H_5AgO_2 = 13 \times 5{\cdot}14 = 66{\cdot}8$$
$$C_4H_7AgO_2 = 16 \times 5{\cdot}14 = 82{\cdot}2$$
$$C_5H_9AgO_2 = 19 \times 5{\cdot}14 = 97{\cdot}7, \text{ \&c.}$$
$$C_6H_5CAgO_2 = 20 \times 5{\cdot}14 = 102{\cdot}8$$
$$C_4H_4Ag_2O_4 = 17 \times 5{\cdot}14 = 87{\cdot}4.$$

ORNITHURIC ACID.—Prof. Jaffe, of Königsberg, in the course of experiments on the transformation of organic bodies on passing through the digestive organs of fowls, has obtained a new acid in a way decidedly different from the usual methods of chemical synthesis. Benzoic acid, C_6H_5COOH, which has been given to birds, is found to be entirely changed by passing through their organisms into a new and well-defined acid, which crystallises in colourless needles, forms a series of salts, and receives the name ornithuric acid. It appears to arise from the combination of benzoic acid with a base $C_5H_{12}N_2O_2$, present in the system, and which can be separated from ornithuric acid by treatment with hydrochloric acid. The formation is as follows:—

$$2C_6H_5COOH + C_5H_{12}N_2O_2 = C_{19}H_{20}N_2O_4 + 2H_2O.$$

DISTILLATION OF ORGANIC LIQUIDS BY MEANS OF STEAM.—Prof. Naumann, of Giessen, describes, in a recent series of papers in the *Berichte der deut. chem. Gesellschaft*, the results of his observations on the phenomena attendant on the passage of steam through organic liquids. As is well known to the experimental chemist, aqueous vapours, on passing through a liquid, carry with them frequently large portions of the latter, even when it boils at a temperature far above that of water—aniline, for example, at 180°. The process also is one of every-day occurrence in the organic laboratory, being used for the purpose of separating such liquids from their impurities. Prof. Naumann has studied in this connection liquids both specifically lighter and heavier than water, as well as liquids boiling below and boiling above 100° C., recording the physical phenomena produced by the passage through each of a regular current of steam. In all cases he finds them obeying a few invariable laws, viz., 1°. For every mixture of a liquid with water there is a constant boiling-point, which is below that of the lower boiling liquid ·2°. A constant ratio exists between the respective quantities of the two liquids found in the distillate ·3°. The temperature of the distilling vapours is always slightly higher than that of the mass of liquid. From among the numerous results the following will convey a general idea of the experiments. The first column contains the boiling points of the respective liquids, the second the temperature of the liquid while steam is being passed through it, and the last the number of cubic centimetres of the liquid found in the distillate for every 100 c.c. of water:—

Benzene	79°·5	68°·5	8·5
Toluene	108·5	82·4	21·2
Xylene	135·5	89	44
Nitrobenzene	205	98·5	14

An attempt was made to discover a connection between the molecular weights of the three first hydrocarbons of the aromatic series and the respective quantities of these liquids in the distillates, but without success. While studying the relations of the numbers yielded by the experiments, Prof. Naumann finally discovered that all the liquids obeyed a general fixed law, viz., *when a liquid is distilled by means of steam, the ratio between the volumes of the liquids and the water in the distillate, expressed in multiples of their molecular weights, is equal to the ratio between their vapour-tensions at the temperature at which the distillation occurs.* It is at once evident that by the discovery of this law the chemist is placed in command of a most valuable auxiliary for determining the constitution of a variety of compounds at present to a certain extent doubtful. The law holds equally good for any liquid the vapour of which is used instead of that of water.

GEOGRAPHICAL NOTES

EARLY AFRICAN EXPLORER.—Don Marcos Ximenez de la Espada of Madrid is now having printed a document of extraordinary interest for geographical science, viz., an account of the travels of an unknown missionary, of the fourteenth century, which Don Marcos has recently discovered. The enterprising author, in the years from 1320 to 1330, undertook extensive travels in Africa, not only along the west coast to Sierra Leone and thence to Dahomey, but also, it is stated, from the mouth of the Senegal river straight across the interior of the great continent. He visited the Soudan States, got as far as Dongola, and thence proceeded down the River Nile, finally reaching Damietta.

AFRICAN EXPLORATION. — In reply to a question from Mr. H. Samuelson last Friday in the House of Commons, the Chancellor of the Exchequer stated that it was not the intention of Government at present to devote any public money to African exploration. We can hardly expect that they would in the present state of public affairs; and even if they could it would be difficult to see in what direction they could take action. There are many expeditions of various kinds in the African field at present, working away with little or no connection with each other; even the International African Association has not been able to organise them, but is simply sending out more expeditions. There seems to us to be considerable waste of power and resources here.

MR. STANLEY.—The Geographical Society's dinner to Mr. Stanley is to take place on February 9. Arrangements are being made to accommodate the Fellows and

their friends in St. James's Hall, but as the hall holds only 2,000, and as there are between 3,000 and 4,000 Fellows, we suspect, making all allowances, that many hundreds will be disappointed. Why does the Society not boldly take the Albert Hall and admit the outside public at a moderate charge? We are sure there would be a balance over after clearing expenses.

BERLIN GEOGRAPHICAL SOCIETY.—The Berlin Gesellschaft für Erdkunde celebrates on April 27 and 28 the completion of its fiftieth year. The festival committee, consisting of Baron v. Richthofen, Dr. Nachtigal, Dr. Jagor, and other well-known explorers, have issued invitations to all the geographical societies of Germany and Austria to send delegates. It is expected that over a thousand will be present at the closing dinner. This society, founded by Alexander von Humboldt and Karl Ritter, has manifested from its commencement a vitality and energy second to none of the European geographical societies, and forms in Berlin a favourite gathering-place for the leading minds in all departments of science. Its membership numbers at present 700.

AUSTRALIA.—An exploring party sent into the interior from Port Darwin, North Australia, under the leadership of Mr. Sergison, has returned to the latter place, and reports that in the vicinity of Victoria River, which runs into the Queen's Channel on the west coast of the Northern Territory, as well as near Fitzmaurice River, which flows more to the north, and near Daly River, which runs into Anson Bay, it discovered land with excellent soil, with a comparatively cool climate, and with numerous creeks in every direction.

ARCTIC EXPLORATION.—The preparations for the Dutch North Polar Expedition are being actively continued, as the expedition is to sail in May next. The first and principal halt will be made at Spitzbergen. The erection and fitting up of a station for meteorological observations is reserved for a future expedition; the present one, however, is to select the place best adapted for a station of this nature. 10,000 florins are still wanting to cover the expenses of the expedition.

CANADA.—On December 22 last a Canadian Geographical Society was founded at Quebec. The principal aim of the society will be to obtain a thorough knowledge of the geography of Canada.

NOTES

THE distribution of the prizes for 1877 by the French Academy of Sciences took place on January 28 under the presidency of M. Peligot. For the two great prizes in mathematics and in physical science no memoir worthy of mention has been sent to the academy. The subjects were very limited in their scope and it is said that the academy proposes to alter its system and confine itself to giving its highest prizes to independent workers irrespective of the subject-matter of their work. Among other prizes awarded we at present mention the following:—The Plumet prize was taken by M. de Freminville, for his improvements in marine steam engines; the Fourneyran prize was awarded to M. Malet, for tramway steam-engines, as used from Bayonne to Biarritz; the Lalande prize in astronomy has been rightly awarded to Prof. Asaph Hall, the discoverer of the satellites of Mars; the Valz prize to the Brothers Henry, for their celestial maps; the Montyon prize in physiology was awarded conjointly to Prof. Ferrier and MM. Carville and Duret; the Lacaize prize for the best work in physics has been given to M. Cornu, the well-known professor of the Polytechnic School for his determination of the velocity of light by direct measurement; the Breant prize (4,000*l.*) to the discoverer of a cure for cholera has not, of course, been awarded, but the interest of that sum has been given to M. Rendu, for several memoirs of etiology. A copy of Laplace's works, magnificently bound, has been delivered, as usual, in the name of Laplace's deceased wife, to the pupil of the Polytechnic School who has passed the most successful examination. The young laureate for 1877 is M. Dougadot, a native of Carcassonne (Aude), where he was born in 1855.

ON January 11 the centennial of Linné's death was observed in nearly all the cities of Sweden. In Stockholm the Academy of Sciences held a special session, attended by King Oscar, at which Prof. Malmsten delivered an interesting oration on the scientific achievements of the great botanist. At Upsala the occasion summoned together a number of notabilities who listened to an address from the Swedish botanist, Prof. Th. Fries. The university of Lund celebrated the day in a similar manner, the rector issuing, in connection with it, a short sketch of Linné's residence there, and Prof. Ogardh delivering the oration. At Frankfort-on-the-Main the memorable day was celebrated by a solemn meeting of the "Freie deutsche Hochstift," in the Goethe House. The president, Prof. Volger, in a brilliant speech, gave an outline of the life, the mental development, the activity, and importance of Linné, and closed by praising the mental ties which unite all races and nations. The meeting unanimously resolved to send a congratulatory telegram to King Oscar II., of Sweden, which was then sent off, written in the Latin language. An hour later his Majesty telegraphed his thanks in the same language. At Amsterdam, where the great Swedish botanist passed the early part of his life, there was also a Linné celebration on Jan. 10. At the same time an exhibition of objects relating to him, such as manuscripts, medals, portraits, &c., was arranged. Prof. Oudemans delivered the memorial speech.

THE French Scientific Association has issued the programme of its weekly lectures for the next three months, and provides a most promising list of famous names and attractive subjects. Among them we notice Prof. Dumas, "Eulogy on Leverrier;" M. Wolf, "Variability of the Nebulæ," which were given on January 26; M. Cornu, "The Phylloxera," February 2; M. Jamin, "Electric Illumination," February 9; Prof. St. Claire Deville, "Liquefaction of Gases," February 23; Prof. Bert, "Influence of Light on Life," March 9; Prof. Mascart, "Atmospheric Electricity," March 23; M. Tissandier, "The Upper Regions of the Air," March 30; M. Blanchard, "Geographical Distribution of Animals," April 13. The lectures take place at the Sorbonne, and as admission is easily obtained by strangers, they offer visitors to Paris an admirable opportunity of hearing the leading French *savants*. The first meeting, on January 26, was attended by more than 1,000 people, under the presidency of M. Dumas. The proceedings were opened by a report read by M. Milne-Edwards, the president of the Association, reviewing the work done by the Association, which was created by M. Leverrier more than fifteen years ago. It is owing to the assistance lent by the Association that weather-warnings have been so largely popularised in rural France and the agricultural service established by the physicist of the observatory.

JAPAN has an active archæological society, bearing the title of Kobutzu-Kai (Society of Old Things). Its members, numbering 200, are scattered throughout the land, but meet once a month in Yeddo. They consist chiefly of wealthy Japanese gentlemen, learned men, and priests; the latter especially have been the means of bringing before public attention a vast number of ancient objects which have been hidden in the treasures of the temples, or preserved in private families. H. von Siebold, Attaché of the Austrian Embassy, at Yeddo, and a member of the society, has lately published a *brochure*, which will serve as a guide for the systematic archæological study of the land; von Siebold has lately made a most interesting dis-

covery of a prehistoric mound at Omuri, near Yeddo, containing over 5,000 different articles in stone, bronze, &c. In a recent communication to the Berlin Anthropologische Gesellschaft, he describes the origin of the terra-cotta images found in old Japanese burial grounds. It appears that up to the year 2 B.C. it was the custom to surround the grave of a dead emperor or empress with a number of their attendants, buried alive up to the neck, their heads forming a ghastly ring about the burial spot. At the date referred to the custom was abolished, and the living offerings were replaced by the clay figures, which have hitherto attracted so much attention.

THE new ethnographical museum in the Palais de l'Industrie, at Paris, was opened on Wednesday last week, the Minister of Public Instruction pronouncing the opening discourse. Deputations were present from all the learned bodies and public institutions of the city, and general satisfaction was expresssed at the admirable manner in which Baron de Watteville, the director, had accomplished his task of organisation and arrangement.

THE Bolton Corporation have just adopted plans for the Chadwick Museum to be erected in the Bolton public park at a cost of 5,000*l*. The amount was left by the will of Dr. Chadwick for this purpose upon condition that the Corporation provided a site. The architect is Mr. R. K. Freeman.

PROF. W. M. GABB writes as follows from Puerto Plata, Sto. Domingo, December 29:—In the issue of November 1 you quote a Paris correspondent of the *Times*, who says that the Madrid people deny the authenticity of the recent finding of the remains of Columbus safe in the Cathedral of Santo Domingo. Of course the Spaniards are not willing to acknowledge that they were hoaxed, but the fact is nevertheless beyond dispute. The remains of Christopher Columbus are to-day in Santo Domingo. Unfortunately I am not able now to send you the full data. Suffice it to say that the chain of evidence is complete and has been verified with all possible precaution. The cheat was perpetrated by a then member of the "Cabildo," who had the knowledge, the tact, and the unscrupulousness to perpetrate it successfully. The whole consular corps, all the Government officials, and all the better class alike of natives and foreigners at the time in Santo Domingo city are witnesses of the authenticity of the "find."

ON Monday afternoon a powerful shock of earthquake was felt in the island of Jersey. It was so strong as to cause houses to totter and bells to ring. Its course was from east to west. There was at the time a heavy gale from the south-west in the English Channel. At 11·55 A.M. the same day a shock, lasting about four seconds, was felt at Eastern Alderney. No doubt it was the same earthquake which was felt at Brighton, Blackheath, Fareham, and St. Leonards, as reported in yesterday's *Times*, and at Paris, Havre, and Rouen, as stated by the *Times* Paris correspondent. Mr. Dobson, writing to us from the Royal Victoria Hospital, Netley, Southampton, states that the first shock occurred there at seven minutes to twelve o'clock exactly, and lasted about five or six seconds. It was sufficiently strong to cause the door to shake with some violence, and many objects in the room continued to vibrate for a considerable time. The second shock occurred a few seconds afterwards, but lasted for a much shorter period. A shock was felt at Lisbon on Saturday, being the third shock during the present winter.

A CIRCULAR signed by Mr. Justin Winsor, librarian of Harvard College, Cambridge, Mass., informs us that it is proposed to issue by subscription a catalogue of scientific serial publications in all languages, which has been prepared by Mr. Samuel H. Scudder, librarian of the American Academy of Arts and Sciences, and formerly librarian of the Boston Society of Natural History, and well known for his various scientific publications. This work, which has double the extent of any existing list of the like kind, aims to include all society transactions and independent journals in every branch of natural, mathematical, and physical science, excepting only the applied sciences—medicine, agriculture, technology, &c. The different institutions or periodicals are arranged under the towns in which they are established or published, and the towns follow an alphabetical order under their respective countries. Cross references are given wherever desirable. The work will be printed in large octavo, will extend to almost 300 pages, and will be delivered, bound in cloth, to subscribers at four dollars the copy. Other copies will be printed on one side of the leaf—to be cut up for catalogue use—and will be delivered in folded sheets at five dollars the copy. Further details may be obtained from Mr. Winsor.

A SECOND edition of Dr. M. Foster's "Text-Book of Physiology," has been published by Messrs. Macmillan and Co. The work has been revised and enlarged, and a number of figures of instruments has been introduced.

IN a recent paper to the Göttingen Society of Sciences, M. Grinitz has compared what data he could obtain regarding the effects of the earthquake at Iquique on May 9 last year. Among other points, it appears that the wave travelled from Iquique to Hilo, in Hawaii, a distance of 5,526 nautical miles, in fourteen hours; which is at the rate of 670 feet per second. From this velocity the average depth of that portion of the ocean traversed can be calculated by Airy's or Russell's formulæ; it is found to be 2,324 fathoms. The wave had an unbroken course to Hilo, but not so to Honolulu, as it encountered the islands of Hawaii, Maui, &c. The average velocity to Honolulu was 654·5 feet per second; and the average sea-depth inferred is 2,219 fathoms. The corresponding numerical data for Apia, Lyttelton, Uskaroa, in New Zealand, Komaishi, in Japan, and other places, are given. (For the last-named a velocity of 679 feet per second was obtained.) On comparison with Hochstetter's results for the earthquake of 1868, and with direct sea-measurements there is seen to be a very fair agreement. Hochstetter's assertion is, on the whole, confirmed, that the velocity of the earthquake wave and the lunar tide wave are identical.

WE have received from Messrs. Hardwicke and Bogue the first volume of their illustrated publication, *Industrial Art*, a monthly review of technical and scientific education at home and abroad. We have carefully examined the work and can say that the text and illustrations run each other very hard for carrying off the palm of excellence. We are glad to gather from the evident success of the venture that the time has arrived when scientific matter is regarded as the natural and necessary accompaniment of a complete reference to art matters. The articles on technical education in France, Austria, and Germany are thoroughly well done.

WE are glad to be able to point to another instance of a collection of the papers of a scientific man during his lifetime. Following hard upon the appearance of Dr. Frankland's collected papers Dr. Lloyd, of Dublin, has published a volume of 500 pages (Longmans) containing his memoirs, reports, and addresses given from time to time, from his classical paper on Conical Refraction to his address delivered before the British Association in 1857. The volume is a very valuable one for a scientific library, for at different times Dr. Lloyd has directed his attention to optics, terrestrial magnetism, and meteorology, and not only have we here the original papers but a series of reports on the progress and present state of physical optics extending over nearly 150 pages somewhat after the style of Verdet's introductions to the various parts of his work.

THE first general meeting of the Institute of Chemistry of Great Britain and Ireland will be held at the rooms of the Chemical Society, Burlington House, Piccadilly, to-morrow, at 4 P.M., to receive the report of the Council. A balance-sheet will also be presented by the treasurer.

WE have received one more evidence of the revival of activity in Italy, in the shape of the first number of a new weekly journal, *La Rassegna Settimanale di Politica, Scienze, Lettere ed Arte*, in which a fair amount of space is devoted to science. It is published at Florence.

THE following is a simple method recommended by Dr. Günther of Berlin, of observing the reversal of the coloured lines of flame-spectra. A thin platinum wire about five ctm. long, is fixed with one end in a glass tube (as holder), and one or two ctm. from the glass it is bent round to a right angle, and inserted in the envelope of a Bunsen flame, so that the free end, held vertical, is heated to a white glow. Into the diametrically opposite part of the flame-sheath is brought a sodium salt. This colours the flame. You then look through a weakly-dispersing prism (the combinations used for direct vision spectroscopes serve best), and through the sodium flame, towards the glowing wire. Two things are observed, (1) the spectrum of the monochromatic sodium flame, which appears in the form of the flame; (2) the spectrum of the glowing wire, which appears as a coloured band, but is broken by the dark D-line. Other metallic spectra may also be shown in this way; only care must be taken that the coloration of the flame be very intense.

AT Hanover the skeleton of a mammoth has just been found, through some excavations which are being made for waterworks near the Ricklinger Beeke. At present only the skull and a tusk have been brought to light, the latter having the circumference of a human leg. The fossils are lying at a depth of six metres.

THE publishing firm of Edouard Rouveyre, in Paris, announces the publication of a voluminous catalogue containing the titles, &c., of all those works, books, pamphlets, &c., which, in the period from October 21, 1814, down to July 31, 1877, have been prosecuted, suppressed, or confiscated, in France. It will appear in five parts, at two francs each.

AT the beginning of the year the new Royal Library of Stockholm, which has now been transferred to the new building at the Humlegaarden, was opened to the public. The new edifice was erected after the design of the architect, Herr G. Dahl, at a cost of 900,000 Swedish crowns. The library, which at the beginning of the present century only numbered 30,000 volumes, now contains 200,000.

A NEW monthly periodical, exclusively devoted to the art of photography and its various branches, is being published since January 1, by Messrs. Ad. Braun and Co., of Dornach. Each number contains an artistic photograph. The title of the new serial is *Die Lichtbildkunst*.

UNUSUALLY severe avalanches are reported this winter from Styria. In the neighbourhood of Hieflau one descended upon a railway train, crushing the carriages, and wounding a number, while at Neuberg another fell upon a châlet containing twelve persons, none of whom escaped.

IN the closing session of the German Chemical Society for 1877, Prof. Kekulé, of Bonn, was elected president, Professors Hofmann and Liebermann of Berlin, Prof. Fehling of Stuttgart, and Prof. Erlenmeyer of Munich vice-presidents. The Society elected also as honorary members the two physicists, Prof. Buff of Giessen, and Prof. Kirchhoff of Berlin, and Dr. Stenhouse of London. At the end of its first decade the German Chemical Society looks back upon a period of rapid growth in numbers and efficiency certainly unparalleled in the history of any society devoted to a special science. These results are due to several marked causes, which could well be imitated by other associations possessing analogous aims, viz., ease of admission, absence of entrance fee and smallness of the annual subscription, simplicity of the statutes, and rapidity and frequency in the publication of the proceedings. The number of members at present is 1,827, showing an increase of 229 during the year. Of these 206 reside in Berlin and 542 outside of Germany and Austria. The membership compares favourably with that of the older sister societies in London (952), and Paris (371). Although the annual payment is so small (15 marks) the society possesses a capital at present of 22,700 marks. During the past ten years the *Berichte* of the society have contained 3,726 communications, covering nearly 14,000 pages. A very complete index to this enormous amount of material will appear during the course of the present year, the compiler of which was selected by competition from among the twenty-nine applicants attracted by the unusually liberal appropriation of 5,000 marks for the work. In addition to the extensive chemical correspondence from America, England, France, Italy, Russia, Sweden, Switzerland, &c., the value of the *Berichte* in the future is to be increased by a complete series of abstracts on all papers appearing in German chemical periodicals. In the last number we notice a very full and interesting sketch of the late Prof. Oppenheim from the pen of Prof. Hofmann, as well as a detailed account of the Chemical Section at the German Association meeting at Munich by Prof. Liebermann.

THE French Academy of Sciences numbers at present 63, three places being vacant by the deaths of Regnault, Becquerel, and Leverrier, the members being divided into eleven sections of six each. There are in addition ten French free academicians and eight foreign associates. The corresponding members, of whom there can be 100, are divided according to their nationality as follows:—France, 32; Germany, 19; Great Britain, 16; Russia, 6; Italy, 2; Austria, 1; Denmark and Sweden, 4; Switzerland, 4; Belgium, 2; United States, 3; Brazil, 1; and there are 11 vacancies.

THE additions to the Zoological Society's Gardens during the past week include a Common Fox (*Canis vulpes*), European, presented by Mr. George Fredericks; two Black Swans (*Cygnus atratus*) from Australia, presented by Capt. W. H. Eccles; a Wood Owl (*Syrnium aluco*), European, presented by Mr. J. E. Liardet; a Common Magpie (*Pica caudata*), a Jackdaw (*Corvus monedula*), European, presented by Mr. G. E. Ladbury; a Hoary Snake (*Coronella cana*) from South Africa, presented by the Rev. G. H. R. Fiske, C.M.Z.S; a Jackass Penguin (*Spheniscus magellanicus*), an Upland Goose (*Bernicla magellanica*) from Chili, two West Indian Rails (*Aramides cayennensis*) from South America, purchased; a Derbian Opossum (*Didelphys derbianus*) from South America, deposited; a Hog Deer (*Cervus porcinus*) born in the Gardens.

RAINFALL IN INDIA

WE have received so many long letters from India on the various aspects of the rainfall question that we must either, from want of space, leave them unpublished, or briefly give the gist of them. We adopt the latter course.

Mr. Archibald sends us a long letter on the seasonal rainfalls of Northern India in connection with the sun-spot period, in which he communicates a few of the principal results obtained from a more detailed and extensive comparison, which the paucity of data at his command hitherto had rendered it impossible to undertake. In the present investigation the registers of eight stations, four in Bengal, and four in the N.W.P. have been employed, and the two seasonal falls of each year, compared (1) for each station separately, and (2) for groups of four and all together, with its position in the sun-spot cycle.

The stations and the periods over which their registers of summer and winter rainfall respectively extend are as follows :—

		Summer rainfall. Years.	Winter rainfall. Years.
Bengal	Calcutta	43	44
	Dacca...	24	25
	Hazaribagh	15	15
	Patna	18	19
N.W.P. ...	Dehra Dun	16	18
	Roorkee	16	18
	Meerut	15	17
	Benares	15	17

"When the deviations from the local average seasonal falls in each year are calculated for each of the above stations separately, and the average taken for each year of the sun-spot cycle, it is found, notwithstanding individual irregularities which occur chiefly in the summer falls, (1) that the winter rainfalls uniformly exhibit a marked tendency to vary *inversely* with the sun-spots at all the stations, (2) that the summer rainfalls show a corresponding tendency to vary *directly* with the sun-spots, which, though strongly marked at the stations in the N.W.P., is scarcely perceptible at the Bengal stations. The result is best seen by combining several of the stations together, and since, owing to the large differences between the actual amount of rainfall at different stations, it is impossible to combine the deviations from the local averages, registered in inches, I have arranged the latter in the form of percentages of their respective averages, then multiplied each percentage deviation in each year of the sun-spot cycle by the number of years corresponding to it at each station, added the several products for the same year, and divided by the sum of the multipliers. By this means each station contributes to the final result in proportion to the extent of its register." Mr. Archibald then gives the tabulated results of combining according to this method (1) the four Bengal stations, (2) the four N.W.P. stations, and (3) all together.

From these tables it is seen that with very few exceptions the inverse relation between the two seasonal falls is strongly manifested throughout, the winter rainfall generally tending to rise above the average in proportion as the summer rainfall tends to fall below the same, and *vice versâ*. The winter rainfall moreover in every case tends to rise to a single maximum exactly coinciding with the period of minimum sun-spot, descending thence to a single minimum which occurs a year or two after the period of sun-spot maximum. The summer rainfall on the other hand exhibits two maxima and minima, and though varying more or less directly with the spots, this variation is principally confined to the N.W.P. stations.

The preceding peculiarities may be rendered still more apparent if we take as a new mean for each year of the cycle the mean of the mean percentage of the year itself together with half that of the preceding and succeeding years.

On the whole it is evident (1) that the winter rainfall throughout Northern India as well as at Calcutta is subject to a periodic variation amounting to nearly 50 per cent. of the average winter fall and corresponding approximately with the inverse phases of sun-spot frequency; (2) that the variation in the summer rainfall, though relatively much smaller, is of an almost exactly opposite character, and that while well-marked in the N.W.P., it is scarcely appreciable in Bengal; (3) that from the last table the cycle may be divided into two distinct portions, viz., the five years preceding, and the six years succeeding, the year of sun-spot maximum. In the former the winter rainfall is excessive and the summer rainfall defective, while in the latter the inverse relations hold, a fact somewhat analogous to the periodic change in the direction of the wind at Oxford and Prague as recently determined by Messrs. Main and Hornstein.

It is scarcely possible at present to indicate the practical deductions that may arise from a consideration of the preceding data. One inference, however, would appear to be immediately deducible, viz., that in any future comparison of the rainfalls of Northern India and other countries similarly subject to distinct seasonal rainfalls, due to distinct aërial currents—such as the monsoon and the anti-trade winds—with the sun-spot period, the summer and winter falls should be compared separately, otherwise it may be found that the combined effect of two opposite seasonal variations renders the resultant variation in the total annual fall very insignificant, or perhaps obliterates it altogether.

With reference to Mr. Hill's letter in NATURE, vol. xvi. p. 505, Mr. Blanford writes that he learns that Mr. Hill was not aware of the existence of his (Mr. Blanford's) paper in the forty-fifth volume of the *Journal* of the Asiatic Society of Bengal (1875), "hence, perhaps, what I cannot but regard as his under-estimate of the extent and validity of the evidence opposed to his view. He discusses the registers of three stations, one in the North-west Himalaya, and two on the dry plains of the Upper Gangetic Valley. My conclusions were based on the data of eleven stations altogether, viz., one in Roorkee, which is also selected by Mr. Hill, and one in Behar; one on the Eastern Himalaya, one on the plateau of Western Bengal, one in Orissa, one in the Andaman Islands, and the remainder in Lower Bengal and Cachar. Moreover, I was careful to eliminate all errors arising from the use of different uncompared instruments; and how necessary such a proceeding is, I illustrated by the remark that I have known sun-thermometers bearing the names of the best London makers differing 10° and 15° in their indications when exposed side by side under similar conditions to the sun. This precaution Mr. Hill has not taken, and I think his results are probably in a great measure due to that fact." Mr. Blanford thinks the sudden changes in the Roorkee register may be accounted for by the fact that the thermometer was twice changed, and the apparent increase in the wind's velocity by the shifting of the anemometers at Benares and Hazaribagh.

With respect to what Mr. Hill has said of the elements of error probably inherent to the method of discussion which Mr. Blanford adopted, while he admits the great difficulty there is in eliminating the effect of disturbing causes, he cannot admit that any *systematic* error was introduced, in the way suggested by Mr. Hill.

Mr. Blanford concludes:—"While on this subject I would direct attention to the importance of regular actinometric observations, of an absolute not merely relative character (such as are shown by the ordinary sun thermometers). The importance of making the solar changes a part of meteorological study is now fully recognised, and it is understood that a trained photographer is about to be sent to India to take photographs of the sun, but if this is so, regular actinometric observation should certainly form a part of the work. The best place perhaps would be Leh, where the atmosphere is remarkably free from haze and dust, which is not the case on the plains of Upper India; nor indeed, in dry weather, on the north-west Himalaya. At Leh, 11,500 feet above the sea, the radiation is most intense. Regular observations with the actinometer carried on for a few years at this place should satisfactorily decide the question of the variation of the sun's heat."

Mr. Blanford also sends a reply to the letter of "Old Madrassee" in NATURE, vol. xvi. p. 519. Mr. Blanford believes that to anyone who has seen or can readily refer to the report on the question of the periodical variation of the rainfall of Madras, it will be abundantly obvious either that "Old Madrassee" can never have seen that report or that he must have misinterpreted its whole purport and argument, and in his references to it, must have trusted to a somewhat unusually treacherous memory.

On the subject of solar radiation and sun-spots Mr. Hill writes that since his article (vol. xvi. p. 505) was written he has gone over the registers of four other stations at which solar thermometers have been in use for five or six years. The former method of treatment is not applicable to these on account of breaks in the registers and changes in the instruments; but adopting suitable methods to compensate for this, the results are as given in the following table, which shows the variation of each year from the preceding one :—

Stations.	1869.	1870.	1871.	1872.	1873.	1874.	1875.	1876.
Benares . .	—	− 1°·0	—	+ 0°·4	+ 1°·9	− 5°·8	+ 3°·3	+ 2°·0
Gorakhpur.	− 0·6	—	—	+ 1·0	+ 0·9	− 0·4	− 2·7	− 1·5
Ranikhet .	—	—	—	+ 0·4	− 3·2	− 6·1	− 3·3	+ 2·9
Ajmere . .	—	− 2·0	+ 2·9	+ 1·7	+ 12·1	+ 3·2	− 4·7	− 0·3
Mean . .	− 0·6	− 1·5	+ 2·9	+ 0·9	+ 2·9	− 2·3	− 1·8	+ 0·8

Owing to inequalities in the number of months combining to

give the averages in the table, and to variations in the number of clear days in each month, the changes from year to year are very irregular, but on the whole there is a decided increase from 1870 to 1876. The sudden fall from 1873 to 1874 must be attributed, Mr. Hill thinks, to the greater diathermancy of the clear air at three of the stations in the former than in the latter year. It is worthy of note that 1873 was a very dry year at all the stations, but that 1874 was much wetter than usual except at Ajmere, where it was drier than 1873. At this station the solar radiation temperature shows a rise instead of a fall between 1873 and 1874.

With regard to the change of anemometer referred to by Mr. Blanford, Mr. Hill says that fortunately, in the present case, any other pair of stations, such as Madras and Vizagapatam, will do as well. With reference to the possible variation of the winter rain of Europe according to the supposed variation in the force of the anti-trade, Mr. Hill notices that the rainfall of London shows such a variation, though not very clearly. He adduces some figures in support of this.

In Mr. Hill's paper, vol. xvi., the word *minimum*, p. 505, second column, eighteenth line from bottom, *exact*, same column, third line from bottom, and *commutative*, p. 506, first column, fourth line from bottom, should be *maximum*, *excess*, and *cumulative* respectively.

Mr. Hill also writes that the large double oscillation in the decennial period of rainfall in Southern India, pointed out by Mr. J. A. Broun, in NATURE, vol. xvi. p. 333, will probably be found to exist in other parts of the country, including the north. One of the longest continuous registers of rainfall in existence for any station in Upper India is that kept by the G.T. Survey Office at Mussoorie, in the Himalayas, lat. N. 30° 28′, long. E. 78° 7′, altitude 6,500 feet. The rain has been recorded since 1854, but only during the rainy season, May to October, inclusive; and the register down to 1873 has been already published by Mr. J. B. N. Hennessey, in the *Proc. R.S.*, vol. xxii. No. 152. Mr. Hennessey's table, extended down to the present year by means of a register kept by the Civil Surgeon, gives a general mean for the twenty-four years of 83·2 inches, and an absolute range of no less than 104 inches.

When the yearly rainfalls are arranged in series of two, three, &c., up to twelve years each, beyond which number it is impossible to extend the series without taking as representative the uncorrected falls of single years, it becomes evident that the great periodic oscillation that underlies the irregular variations must complete its cycle in from nine to twelve years, for the 9-, 10-, 11-, and 12-year series, all show a large amplitude of oscillation, and in the 11-year series the maximum and minimum occur at nearly opposite phases of the cycle. It is also evident that in the 6-year series the conditions are the same, the only difference being that the amplitude does not appear quite so great.

Calculating the coefficients of the equation of sines for the ten-and-a-half-year period, as Mr. Broun has done, we get for the variable part of the Mussoorie rainfall—

$$y = 11{\cdot}4 \sin \theta + 14{\cdot}0 \sin (2\theta + 337^\circ).$$

This may be compared with the equations given in Mr. Broun's article for Madras and Trevandrum,[1] for in all these equations $\theta = 0$ for the years 1838·5, 1849, 1859·5, 1870, &c. The difference of the two angles, 259° and 337°, causes a difference of more than a year in the epochs of the maxima and minima of the secondary oscillations, otherwise there is a wonderful similarity between the formulæ for two such widely-separated stations as Mussoorie and Trevandrum.[2]

Mr. Hill thinks it most likely that the oscillation of the five-yearly period is either purely accidental or the effect of some cause not yet understood.

Mr. Archibald, writing on the subject of Cyclone Generation, directs attention to an exceedingly interesting article in the *Pioneer* of October 30, entitled "A Cyclone Study," in which the author brings forward some very strong additional proof in confirmation of the "condensation theory" held by Messrs. Eliot and Blanford as opposed to the "parallel wind theory" advocated by Drs. Hann and Thau, and Messrs. Meldrum and Willson. After giving a clear account of the main points of difference between the above theories, the writer then proceeds to trace the history of the last cyclone in the Bay of Bengal, the Madras cyclone of May last, from its origin to its final disappearance, pointing out certain circumstances as giving strong support in favour of the condensation theory, and as completely disposing of the parallel wind theory—at all events as far as regards this particular cyclone.

[1] Viz.: $y = 5{\cdot}4 \sin (\theta + 50^\circ) + 4{\cdot}6 \sin (2\theta + 252^\circ)$,
and $y = 5{\cdot}6 \sin (\theta - 17^\circ) + 8{\cdot}4 \sin (2\theta + 259^\circ)$.

[2] The above equation for Mussoorie gives the maxima in 1860·3, 1870·8, &c., and the minima in 1857·2, 1867·7, &c. The first term alone would give the maxima in 1860·1, 1870·6, &c., and the minima in 1855·9, 1866·4, 1876·9, &c.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The Mathematical Tripos' list this year contains ninety-four names. There are thirty-one classed as Wranglers, thirty as Senior Optimes, twenty-nine as Junior Optimes, and four Ægrotant. The Senior Wrangler is Mr. Ernest William Hobson, Scholar of Christ's College, eldest son of Mr. W. Hobson, proprietor and editor of the *Derbyshire Advertiser*. He was educated at Derby School, and in 1874 obtained an open scholarship at Christ's College. During his undergraduate career he obtained the first place among the students in the college examinations in mathematics. His college tutor was Mr. Peile, and his private tutor Mr. E. J. Routh, of St. Peter's College. Next to him are Mr. John Edward Aloysius Steggall, scholar of Trinity College, and Mr. Christopher Graham, scholar of Caius.

During the present term three courses of lectures on chemistry will be delivered. A general course by Mr. Main at St. John's College; a course by Mr. Lewis at Downing College; and a course on the non-metallic elements by Mr. Pattison Muir at Caius College.

MUNICH.—The university is becoming in point of numbers one of the foremost in Germany. The calendar for the present year shows an attendance of 1,360, of whom 1,014 are from Bavaria and 346 from other countries. In the theological faculty there are 82, in the legal 387, in the medical 341, in the philosophical (history, philology, &c.) 246, (science) 151, together with 136 pharmaceutical chemists, and 17 in forestry and agriculture. The corps of instructors numbers 114. The university, although but fifty-two years old, has been well supported by the State, and possesses a large variety of laboratories, cabinets, &c., and a library of 20,000 volumes.

SOCIETIES AND ACADEMIES

LONDON

Royal Astronomical Society, January 11.—Dr. Huggins, F.R.S., in the chair.—A paper by Mr. W. F. Denning on suspected repetitions or second outbursts from radiant points, and on the long duration of meteor showers, was read, showing that a radiant in some cases continues active during three or four months, and sometimes a second outburst occurs after an interval of six months, so that meteors may be seen coming from the same radiant at opposite sides of the earth's orbit. Capt. Tupman commented on this paper at some length, and pointed out some of the difficulties these conclusions presented.—Dr. Wentworth Erck read a paper on a combined position and setting circle, rendering the declination circle unnecessary on large Newtonian equatorials. He also showed a small and singularly portable equatorial mounting, and read a note on a spectroscope made by Mr. Grubb for Prof. Young, showing certain improvements. Mr. John Browning admired the ingenuity of these, and explained which of them were new and which were not.—Mr. A. A. Common read a note on the satellites of Mars and Saturn.—A note was read describing the failure of the Melbourne telescope to deal with the satellites of Mars.—Mr. S. Waters read a paper on the distribution of the fixed stars in space.—Mr. Christie read a paper on specular reflection from Venus, the purport of which was that his recent observations of the planet with the polarising eye-piece emphatically corroborated those made in 1876. By means of this eye-piece the light of the disc is gradually reduced; and he found in every examination that the last part of the disc to disappear was situated at a point which was found by calculation to coincide with the point indicated by the theory of specular reflection, thus confirming Mr. Brett's original description of the phenomenon. Mr. Christie had the assistance of Capt. Tupman in his recent observations. Mr. Neison suggested certain other explanations of the appearances described, and after further discussion the meeting adjourned.

Zoological Society, January 15.—R. Hudson, F.R.S., vice-president, in the chair.—A communication was read from Mr.

Andrew Anderson, F.Z.S., containing some corrections and additions to a former paper of his on the raptorial birds of the north-west provinces, read before the Society on March 21, 1876.—A communication was read from Mr. F. Moore, F.Z.S., containing a revision of the genera and species of European and Asiatic lepidoptera belonging to the family Lithosiidæ. The author characterised thirty-eight genera in this memoir, and gave the descriptions of eighty new species.—Mr. A. Boucard, C.M.Z.S., read a paper in which he gave a list of the birds he had collected during a recent expedition to Costa Rica. The number of birds collected during his five months' stay was about one thousand in number, representing 250 species, amongst which were two new to science (*Zonotrichia boucardi* and *Sapphironia boucardi* of Mulsant) and many others of great rarity.—Two papers were read by Mr. G. French Angas. The first contained descriptions of seven new species of land shells recently collected in Costa Rica by M. A. Boucard. The second contained the description of a new species of *Latiaxis* from an unknown locality, proposed to be called *L. elegans*.—A communication was read from Dr. H. Burmeister, containing notes on *Conurus hilaris* and other parrots of the Argentine Republic.—A communication was read from the Count Salvadori, C.M.Z.S., in which an account was given of the birds collected during the voyage of H.M.S. *Challenger*, at Ternate, Amboyna, Banda, the Ké Islands, and the Aru Islands.—Prof. Garrod, F.R.S., read a paper on certain points in the anatomy of the Momotidæ, in which he adduced facts substantiating their affinities with the Todidæ, Alcedinidæ, and other Piciformes. The second paper described the extraordinary structure of the gizzard of the Fijian Fruit Pigeon (*Carpophaga latraus*), in connection with the fruit on which it feeds, that of *Oncocarpus vitiensis*.—A communication was read from Mr. Edgar A. Smith, F.Z.S., containing the description of a new species of Helix from Japan, which he proposed to call *Helix* (*Camena*) *congener*.—A communication was read from the Marquis of Tweeddale, F.R.S., containing an account of a collection of birds made by Mr. A. H. Everett in the Philippine Islands of Dinagat, Bazol, Nipak, and Sakeryok. Six new species were found in this collection and were named *Ceyx argentata*, *Hypothymys cœlestis*, *Mixornis capitalis*, *Dicæum schistaceum*, *D. everetti*, and *Prionochilus olivaceus*.—A second paper by the Marquis of Tweeddale gave the description of a new genus and species of bird from the Philippine Island of Negros, for which the name *Dasycrotapha speciosa* was proposed.

Photographic Society, January 8.—James Glaisher, F.R.S., president, in the chair.—Papers were read by Capt. Abney, F.R.S., on the theory of the destruction of the undeveloped photographic image; by Edward Viles, on the production of enlarged photographs of microscopic objects; and by Edwin Cocking, "stray thoughts on the exhibition."—Capt. Abney in his paper stated the result of experiments undertaken to ascertain the cause of the fading away of the undeveloped image on dry plates by long keeping after exposure. Films of pure silver iodide, and of pure silver bromide, after exposure, were washed with potassium permanganate, potassium bichromate, and chromic acid; with the silver iodide salt, *all* destroyed the image, with the silver bromide salt the last two oxidising agents alone were effective. If this destruction of the image was caused by oxidation of the silver atom, it should also be oxidised by ozone—which experiments showed was the case. Capt. Abney then assumes that the effect of time on the image on a dry plate is to oxidise an atom of each of the molecules forming the image.

Institution of Civil Engineers, January 15.—The newly-elected president, Mr. John Frederic Bateman, F.R.SS.L. and E., delivered an inaugural address. After a passing allusion to the growth of the Institution, which at the end of 1844 numbered only 552 of all classes, now increased to 3,189, reference was made to some of the addresses of the eighteen gentlemen who had previously occupied the presidential chair, mainly for the purposes of comparison. Proceeding to matters more personal to every member of the Institution, the President urged that engineering was but, in fact, the embodiment of practical wisdom; or, in the words of Bacon, "the conjunction of contemplation and action."

EDINBURGH

Royal Society, January 7.—Bishop Cotterill, vice-president, in the chair.—Prof. Blackie read a paper on Mr. Gladstone's theory of colour-sense in Homer, which he completely refuted. A discussion followed, in which Principal Sir Alexander Grant, Bart., the Rev. Dr. Cazenove, Prof. Fleeming Jenkin, Dr. Donaldson, and others took part.—Prof. Tait postponed his paper on the intensity of currents required to work the telephone but mentioned that Mr. James Blyth had obtained good results with telephones in which he had employed discs of copper-wood vulcanised india-rubber paper, instead of the usual iron ones.—Prof. Tait also laid on the table a double mouthed-piece horn for producing chords by two performers on the same instrument.

VIENNA

Imperial Academy of Sciences, November 16, 1877.—On ice in the Danube in Lower Austria, in the winter 1876–77, by the Minister of the Interior.—Researches on the consciousness of place and its relation to the conception of space, by M. Stricker.—On the temperature of Vienna according to 100 years' observations, by M. Hann.—On the phanerogam flora of the Hawaii Islands, by M. Reichardt.

November 22, 1877.—On a partial differential equation of the first order, by M. Hocevar. The laws of the individuality of the planets of our solar system; attempt to establish a general theory, by M. Lehmann.

December 6, 1877.—The velocity of propagation of spark waves, by MM. Mach, Tumlirz, and Kögler.—On the application of Doppler's principle to the progressive motion of luminous gas molecules, by M. Pfaundler.—On some problems of the theory of elastic reaction, and on a new method of observing vibrations by reading of mirrors, without loading the vibrating body with a mirror of considerable size, by M. Boltzmann.—Determination of surfaces any of whose parts, from two fixed points, are projected through cones the apertures of which are in a given proportion, by M. Weyr.—On mononitrobrenzcatechin, by M. Benedikt.—Size and position of the optical axes of elasticity in gypsum, by M. von Lang.—On the orbit of the planet Laurentia (162), by M. Zelbr.

PARIS

Academy of Sciences, January 21.—M. Daubrée in the chair.—On account of the death of MM. Becquerel and Regnault, the *séance* was adjourned. The funeral of M. Becquerel took place the same day, that of M. Regnault next day. Discourses on the former were pronounced by MM. Fizeau and Daubrée; on the latter by MM. Debray, Jamin, Daubrée, and Laboulaye. [These are reported in the *Comptes Rendus* for the week.]

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